

MINUTES FROM THE EPA/SCIENCE ADVISORY BOARD
Environmental Economics Advisory Committee Meeting
November 18, 1998

PURPOSE: The Environmental Economics Advisory Committee (EEAC) met to continue its review of the EPA draft economic analysis guidelines. The EEAC also engaged in a dialogue with Mr. David Gardiner, Assistant Administrator for Policy, Planning, and Evaluation, U.S. Environmental Protection Agency. In addition, the EEAC discussed the possible reinitiation of the Pollution Abatement Costs and Expenditures (PACE) survey. The meeting was announced in the Federal Register at FR Vol. 63, No. 207, Pages 57296-57298 (10/27/98) (see Attachment A). An agenda is included as Attachment B.

LOCATION: The meeting was held at the Ramada Hotel Old Town, 901 N. Fairfax Street, Alexandria, VA.

PARTICIPANTS: The following SAB members and staff participated in this meeting of the EEAC: Drs. Robert Stavins, Dallas Burtraw, Trudy Cameron, Dale Jorgenson, Paul Joskow, Catherine Kling, Richard Revesz, Hilary Sigman, and Kip Viscusi. A committee roster indicating members who were present and those absent is included as Attachment C. EPA Staff and persons from the public who attended the meeting are indicated on the sign-in sheets (Attachment D).

MEETING SUMMARY: The meeting followed the published agenda. A summary of the activities follows.

(9:00) Welcome and Introductory Remarks; Dr. Robert Stavins, Harvard University

Dr. Stavins called the meeting to order and welcomed the committee and observers at 9:05 a.m. In the interest of public awareness, Dr. Stavins asked the members to introduce themselves and to remark on any potential for a conflict of interest that might exist with the day's agenda items and the members' interests. No conflicts of interest were mentioned; however, a number of members remarked on their research interests and funding sources that they judged to be in the interest of the attendees at the meeting.

Dr. Stavins noted the following membership changes that occurred on 10/1/98. Those members who have served three complete terms and who are leaving EEAC membership include Dr. Robert Repetto, Dr. Kip Viscusi, and Dr. Charles Kolstad. Dr. Richard Schmalensee has resigned from the EEAC in view of his new responsibilities as the Dean of the Sloan School of Management at MIT. Dr. Stavins thanked the retiring members for their valuable service and welcomed the new members to the EEAC: Dr. Dallas Burtraw, Dr. Paul Joskow, Dr. Jason Shogren (not in attendance), and Dr. Hilary Sigman.

Dr. Stavins also noted the September approval of the EEAC's advisory on economic research topics and noted the major items on the agenda.

(9:18 - 10:10 a.m.) Pollution Abatement Costs and Expenditures survey; a discussion of the possible reinstitution of the survey. Dr. McGartland (EPA) and representatives from Bureau of the Census and the Bureau of Economic Analysis; EEAC Members

Dr. Stavins noted the concern within the economics research and environmental policy communities two years ago when it was first understood that the annual Pollution Abatement Costs and Expenditures (PACE) survey in the Department of Commerce was going to cease. He introduced Dr. Al McGartland, USEPA/OP, and Dr. Elinore Champion (Census) who discussed recent developments that could lead to the reinstitution of such a survey.

Dr. McGartland provided supplemental written materials to the EEAC to inform the discussion (Attachment E1-Re-instituting the PACE Data Series: Issues for Discussion; E2--Workbook and Answer Sheet for the PACE Survey; and E3--Selected language from the Omnibus Appropriations Bill from the 10-14-98 issues of the *Congressional Record* also referred to as the "Stevens Report" language). Past surveys have been used by the Agency and others to support the analysis of regulatory costs. New surveys would be useful in responding to the "Stevens Report" requirement for cost and benefit reporting by 2000. Issues identified by Dr. McGartland included 1) how one might reconcile the baseline as defined in PACE with EPA's need to measure costs attributable to federal environmental regulations and what an appropriate definition of a baseline might be; 2) what should be included in costs and whether costs can be apportioned across a diverse set of subcategories; 3) how pollution prevention expenses could be identified and tracked; 4) improvement of the reliability of PACE data; and 5) the adequacy of sector coverage of the survey. The Agency has no final agreement to support PACE; however, it is looking into how it might find the funds that would be offered as its share of the survey cost (survey would cost a minimum of \$1.0 million per year).

EEAC comments were directed at issues of 1) the appropriateness of spending a substantial portion of the overall economics extramural budget on reinstituting PACE; 2) whether PACE data are useful to the Agency; 3) whether there was a need to change the survey design if PACE is resumed; and 4) the importance of being able to link PACE data to other survey data in order to conduct analyses that suggest answers to whether expenditures were in response to regulations or ordinary production upgrades.

The EEAC agreed that it would draft an Advisory to the Administrator in support of resumption of such a survey, but also decided not to comment on the design issue at this time. Dr. Stavins appointed Dr. Burtraw to chair a subgroup to draft the commentary. Joining the group to prepare the draft would be Drs. Joskow, Jorgenson, and Viscusi.

10:10-10:25 BREAK

(10:25-11:30) An Interaction with the EPA Assistant Administrator for Policy, Planning, and Evaluation, Dr. David M. Gardiner, U.S. Environmental Protection Agency

Mr. David Gardiner, Assistant Administrator for Policy, Planning, and Evaluation, US E.P.A., provided his perspectives on the increasing need for and importance of economic analysis in environmental policy formulation. He complimented the EEAC on its past support and noted the continuing need for EEAC assistance and review of Agency efforts to improve economic analysis in environmental public policy making.

Mr. Gardiner noted the four Agency activities that form the building blocks for EPA's efforts to improve economic analysis. These include: 1) an EPA staff survey of economic research needs and the development of an economic research plan (now close to completion), 2) a series of interactions with outside economists on critical topics in order to bring broader thinking and research efforts into Agency consideration of these issues, 3) consideration of providing support for the resumption of PACE, and 4) the development of economic analysis guidelines.

Mr. Gardiner noted a number of issues that have emerged from the consideration of budget priorities by EPA senior managers over the last two years. At the top of the list are issues associated with clean air, climate change, non-point water pollution, and information collection and dissemination. Public interest in information has grown to the point that the EPA website is now accessed more than one million times a day. The quality of data being accessed is a growing concern. EPA is reorganizing much of its information resources program because of the growing need for disseminating high quality data. Mr. Gardiner noted that it is helpful to consider how economic analysis can be brought to bear to help the agency understand these priority issues. EPA's economics efforts will move towards new mechanisms for surveying the economic aspects of emerging environmental problems, periodic updates to the economic analysis guidelines, understanding the technical issues involved with quantifying benefits (especially within the context of the Stevens Report requirements), and influence of environmental regulation on technological advance. In response to a question regarding future EEAC projects, Mr. Gardiner noted that these key areas are those from which specific future EEAC projects will be derived. More specifically, the EEAC might become involved with the issue of ancillary benefits from co-control of specific environmental risks, policies that promote technology innovation, retrospective looks at the costs and benefits of Agency programs in waste and water, and looking into environmental issues at the state level--the level at which most programs are implemented.

In response to EEAC interest, Mr. Gardiner commented on the reorganization that is now being considered for EPA. Changes associated with the agency's information priorities were noted earlier. Often new ideas are developed and tested within the Office of Policy. Later, when they prove out they are moved to other parts of the Agency for routine implementation. However, economics is a function of policy development. It appears that there is strong support across EPA's senior management for retaining a central core of economics expertise at EPA. It is not yet certain where such a function will ultimately reside. Options are to keep it in a separate Assistant Administrator-level, independent office; to move it to the Administrator's office as an Associate Administrator or Staff Office; or to place it within the Office of Research and Development. The bottom line for the Agency is to have this core of expertise help the

Agency to get to where it needs to be regarding quality economic analysis in support of its mission.

EEAC members suggested pros and cons of each placement and indicated a desire to make their views known as supporting a strong core of expertise in one central area that would enjoy an appropriate level of independence. The Committee decided against preparing a letter for the SAB. Instead, Dr. Stavins indicated that he would meet with the Deputy Administrator as soon as possible and discuss the issue with him as an individual and not as a member of the SAB conveying SAB advice.

11:30-11:45 BREAK

(11:30-3:30) Review of the EPA Guidelines for Economic Analysis; EEAC Members, Dr. Albert McGartland, US EPA/Office of Policy (NOTE: This interaction included a working lunch.)

Dr. Stavins introduced the topic by noting that a letter, dated August 4th from Fred Hansen contained the official charge to the EEAC to review EPA's revised, draft "Guidelines for Preparing Economic Analyses." He summarized the actions on this review to date for the sake of new members.

Dr. Al McGartland noted that the Agency had rethought a number of issues and made a number of changes to the guidelines as a result of the discussion that occurred during the August 1998 EEAC meeting (see Attachment F). Some comments did not result in changes because of institutional constraints even though they had merit.

The EEAC then conducted a chapter by chapter discussion of the new draft guidelines. Dr. Stavins was the lead discussant for chapters 1 through 3. No major issues were identified in these introductory chapters.

Drs. Revesz and Stavins were lead discussants for chapter 4, "Regulatory and Non-Regulatory Approaches to Consider." Comments from the committee were directed at the following issues: placement of deposit-refund and bubbling discussions; the fit of topic 4.5 relative to material that preceded it; omission of California's experience with liability rules; permit allocation; design-based versus performance-based standards; and subsidies.

Drs. Burtraw and Kling were lead discussants for chapter 5, "Overview of Economic Analysis of Environmental Policy." Comments from the committee were directed at the following issues: dynamic versus static models; the relationship between benefit-cost analysis (B/CA) and other topics in the chapter which should be thought of as subtopics; the inclusivity of B/CA (aggregate concept) with respect to the ideas of distribution and equity (disaggregated parts); tax interaction effects; baseline issues; policy selections versus technological change; and uncertainty.

Dr. Stavins asked Dr. Warner North, NorthWorks, speaking as a member of the **public** to make his **comments** at this time. Dr. North commented upon the **uncertainty**

issue. He noted that in his twenty-plus years of involvement in risk analysis he had observed that the gap between known and unknown--uncertainty--is large. He sees the source of the problem as EPA's reading of the 1983 risk guidance from the National Academy of Science which leads EPA to carry out risk assessment largely based on animal studies and generating upper bound estimates. He does not think the Economic Analysis Guidelines draft is sufficiently clear in instructing EPA analysts in how to handle uncertainty and the issue of "conservative" risk estimation. The citations on the issue of uncertainty are old in his opinion. He suggests the Agency look at a recent book by Dr. Granger Morgan, a recent NAS report, and the Presidential Risk Commission report as good sources of more appropriate guidance for handling uncertainty. (Dr. North's written comments are contained in Attachment G--Public Comments)

Drs. Cameron and Sigman were lead discussants for chapter 6, "analysis of Social Discounting." Compliments were offered to the Agency on this chapter by a number of members. Comments from the committee were directed at the following issues: dismissing conventional Pareto analysis applicability to discounting; placement of the discussion of partial versus general equilibrium; the cookbook nature of mandatory requirements to consider three specific discount rates; and the impact of lessened uncertainty levels over time on the choice of a discount rate.

Drs. Viscusi, Cameron, and Kling were lead discussants for chapter 7, "Analyzing Benefits." Comments from the committee were directed at the following issues: value of life years and how that might vary depending upon the age of the person involved; cause of death relative to a person's perception of that way of dying; primacy of consumer sovereignty; the amount of discussion of contingent valuation and the studies cited as exemplary in regard to CV; the permissiveness of the guidance on benefits transfer (focus should be on transferring the function not necessarily the value); and the use of point estimates alone in benefits transfer.

Dr. Stavins asked Dr. John Graham, Harvard Center for Risk Analysis, Harvard University, speaking as a member of the public, to make his comments at this time. Dr. Graham's first concern was with the appropriate valuation of mortality risk. He stated that the Agency's "Guidance on 'transfer' of VSL values from safety literature to environmental context is too permissive and possibly misleading. It allows the transfer of a \$5.8 million value of statistical life (VSL) without adjustment. He suggests that evidence in the literature supports requiring "ballpark" adjustments for: 1) longevity (after onset of the health effect), 2) discounting longevity with latency, 3) health-related quality of extra life years, and 4) psychological considerations regarding ways of dying. A second concern was that the guidelines need to **authorize QALY-based** approaches (see Attachment G--Public Comments).

(2:30-2:45) BREAK

Drs. Burtraw, Jorgenson, and Joskow were lead discussants for chapter 8, "Analyzing Social Costs." Comments from the committee were directed at the following issues: efforts to disaggregate cost estimates by control type; deferral to partial equilibrium analysis; the impact of costs on innovation, efficiency, and the track of the

economy; tax interaction effects; dynamic versus static situations; the level of detail in the chapter; and uncertainty. Some members noted that this is the most important chapter in the guidelines and it presents the most opportunity for the document to provide value to EPA. The Clean Air Act section 812 report was suggested as a good example to illustrate techniques discussed in this chapter. It is also applicable to some other chapters.

Drs. Jorgenson, Joskow, and Sigman were lead discussants for chapter 9, "Distributional Analyses: Economic Impact and Equity." Comments from the committee were directed at the following issues: too little discussion of cost pass-through; competition and market power; this chapter is about disaggregation--not about issues that are not included in aggregate measures used in economic analysis; these are ways to focus on pieces of the whole; 'economic impact' is a poor title for one of this chapter's topics; descriptions of environmental laws may be inaccurate. Some members thought that the level of detail in the chapter may be too great.

Drs. Revesz and Viscusi were lead discussants for chapter 10, "Using Economic Analyses in Decision Making." Comments from the committee were directed at the following issues: add information on risk-risk analysis associated with proposed options; and discriminate clearly between cost/benefit and cost-effectiveness.

Additional points made by members include that the Agency needs to keep in mind that guidelines do not substitute for good judgment--this is not a cookbook. Further it is important to recognize that even after an analysis is completed, there is still much uncertainty about the numbers generated. This uncertainty has to be made clear to decision makers and the public. In general, the Committee expressed substantial admiration for the guidelines document and feel that it is developing well.

NEXT STEPS:

The EEAC agreed that the following actions would be taken as a result of this meeting.

a. Economic Analysis Guidelines

The EEAC members' discussions at this meeting are one part of the Committee members' feedback to the Agency. These comments should be written up by each member and submitted to Thomas Miller, EEAC DFO, by December 2, 1998 for inclusion within the meeting minutes. We anticipate that the Agency will consider these reactions from individual members and decide if it wishes to adjust the guidelines further prior to submitting them to the EEAC for final review.

The members should provide their availability for another EEAC meeting during March 1999. A final discussion will occur on the guidelines at that time and the final EEAC advice will be provided in a report that will be delivered to the Agency via the SAB Executive Committee. The comments of EEAC members are included in Attachment H of these minutes.

b. Commentary on the Reinstitution of the Pollution Abatement Costs and Expenditures Survey

The support of the EEAC for reinstitution of a survey will be captured in an Advisory that will be sent to the Agency via the SAB Executive Committee. Drs. Burtraw, Joskow, Jorgenson, and Viscusi will prepare the draft of the substantive points, they will send it to Dr. Stavins who will revise the draft and provide it to Tom Miller for proper formatting, it will then be sent to the EEAC for review, comment and concurrence. A final consensus report will be prepared and sent to the SAB Executive Committee for approval at their January, 1999 meeting.

c. Future Activities

Some candidates have emerged:

1. Redesign of PACE survey
2. Design and execution of "Cost of Clean"
3. Ways to handle dynamic analysis (e.g., climate change) (Dr. McGartland will prepare a memorandum to the EEAC on this issue)
4. Approaches for responding to the "Steven's Report" requirement for EPA

The meeting was adjourned at 3:50 p.m.

I certify that these minutes are accurate to the best of my knowledge.

/ S /

Dr. Robert N. Stavins
Chairman
Environmental Economics Advisory
Committee
November 25, 1998

/ S /

Mr. Thomas O. Miller
Designated Federal Officer
Environmental Economics Advisory
Committee

ATTACHMENTS:

- A Federal Register Notice
- B Meeting Agenda
- C Committee Roster
- D Sign-in Sheets
- E 1-Reinstituting the Pace Data series: Issues for Discussion
2-Workbook and Answer Sheet for the PACE Survey
3-Selected language from the Omnibus Appropriations Bill from the 10-14-98
Congressional Record (Steven's report language)
- F Revised Guidelines for Preparing Economic Analysis; dated November 3, 1998
- G Public Comments --Dr. John Graham
--Dr. Warner North
- H Written Comments of Individual Members of the EEAC:
--Bockstael
--Burtraw
--Cameron
--Kling
--Sigman
--Viscusi

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Dr. John Graham

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December 2, 1998

Professor Robert Stavins
Chair, Environmental Economics Advisory Committee
Science Advisory Board, EPA
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79 John F. Kennedy Street
Cambridge, MA 02168

Dear Professor Stavins:

I appreciated the opportunity to attend the November 18th meeting of the Environmental Economics Committee of the EPA Science Advisory Board. The purpose of this letter is to follow up on my oral remarks and offer some practical guidance concerning revisions of the draft "Guidelines for Preparing Economic Analysis" (November 3, 1998). I have also appended some references that may be useful to agency staff in the process of guideline revision and other activities concerning valuation of health risks.

As you know, I support the Agency's efforts to modernize its guidelines for conducting economic analysis. I was also impressed with a variety of the principles and "tips for analysts" included in the November 3rd draft.

My concerns about the draft are very specific, relating to how "benefit transfers" for human health risks are treated in Chapter 7 (pp. 66-68, 89-97). In my professional opinion, the draft guidance on the monetization of human health risks is too permissive because it sanctions outdated practices and does not compel use of the best available data, insights, and methods in the economics literature. I am particularly concerned about that the draft guidance does not make use of the advances and experience gained in the application of economics to public health and medical practices over the past twenty years.

Accounting for Longevity in Valuation of Mortality Risks

At the bottom of p. 89 (7.61), the draft guidance notes correctly that direct evidence on the monetary value of life extension from environmental policy is not available. Thus, an exercise in "benefit transfer" (pp. 87-88) is required. The guidance that is presented on pp. 89 - 95 does mention some of the important issues in benefit transfer but ultimately allows agency analysts to continue the outdated practice of assigning the same "value of statistical life" or VSL (\$5.8 million) to each "life that is saved", regardless of the degree of life extension (longevity) that is involved.

The failure to address longevity will lead to substantial errors in valuation. For example, the \$5.8 million figure is based on studies of workplace accidents and car crashes where the typical fatality results in a loss of 30 to 50 (undiscounted) life years (Viscusi, 1993). It is not appropriate to apply this estimate to acute or chronic cases of fatal disease that involve losses of life expectancy in the range of <1 to 15 years.

Rough estimates of life extension can be derived from use of life tables and the average age of death from the respective causes of death. Yet it is important to keep in mind that the objective is not to devalue the lives of people who are older. Life extension is a function of a variety of population characteristics including baseline health status and other risk factors for premature death (e.g., smoking, blood pressure, prior heart attack, and so forth). The presence of serious co-morbidity in the target population can be a more significant variable than age per se in determining the extent of life extension from policy change. The toxicological and epidemiological communities are rapidly developing information that will permit longevity estimates that depend on numerous factors in addition to age.

Failure to Correct for Latency, Morbid States, and Psychological Trauma

The draft report contains insufficient guidance on several other important issues in the valuation of mortality risks. The draft guidance fails to make proper recommendations on latent mortality effects, where the issue of time preference must be addressed. The draft report also neglects to offer guidance on how to value additional longevity that occurs in a morbid state, where quality of life is diminished. Again, it is not appropriate to apply the same VSL to the preservation of poor quality life years that is applied to the preservation of healthy life years. Moreover, the draft guidance is so permissive that it would permit agency analysts to ignore the differential psychological impacts of involuntary and voluntary exposures, even though the public appears to place a premium on the prevention of involuntary exposures.

In light of these problems with the draft guidance, I would like to offer practical suggestions concerning how the draft guidance should be changed.

1. When transferring VSLs to the environmental context, at least a rough adjustment for longevity is expected in the main analysis of benefits. Longevity gains should be expressed in present value (using an appropriate real discount rate), prior to making the VSL adjustment.

Discussion

There appeared to be consensus on this issue at the SAB meeting. Professor Viscusi's recommendation would certainly satisfy my concern on the longevity issue.

2. If VSLs are an important variable in benefits analysis, sensitivity analyses should be conducted to reflect at least three plausible approaches to valuing life years at different points in the life span.

Discussion

It is well established that the value of saving a statistical life (VSL), based on the principle of consumer sovereignty, will vary over the life span, rising to a peak in the middle of the lifespan and falling in advanced ages (Arthur, 1981; Shepard and Zeckhauser, 1984; Rosen, 1988). Yet it is not clear empirically how life years at different points in the lifespan are valued, given that the same life year (e.g., the 70th year) may be valued differently depending upon whether the consumer happens to be 30 years old or 60 years old.

The preferred approach in traditional benefit-cost analysis is to value life years as informed consumers would value them, given their current ages and reference points. The limited evidence that exists suggests that senior citizens may assign somewhat larger values to a typical remaining life year than middle-age citizens assign to a typical remaining life year (Jones-Lee et al, 1985). An important limitation of this approach is that the relevant studies, which typically control for annual income but not wealth (asset position), may be confusing a wealth effect for a premium on life years at older ages. It is also possible that subjects in these studies do not understand the longevity issue and thus offer values that are not adequately responsive to the extent of longevity gain, a type of insensitivity that has been observed in valuation studies involving other quantitative goods (Baron, 1997; Frederick and Fischhoff, 1998).

A second approach, the standard in public health and medicine, is to value equally each year of life in good health, called a quality-adjusted life year (QALY), without regard to age, gender, wealth, or other population characteristics (Zeckhauser and Shepard, 1976; Gold et al, 1996). The strengths of this approach are that it is simple to implement, is ethically appealing, and has some grounding in expected utility theory (Garber and Phelps, 1997). A limitation of this approach is that it may not be consistent with consumer sovereignty as applied in the traditional theory of benefit-cost analysis, where valuations are made by citizens at their current ages and demographic reference points.

A third approach, widely employed in the field of international health/development and called the "disability-adjusted life year" (DALY), is to value life years in the middle of the lifespan more heavily than life years at the beginning and the end of the life span (Murray, 1994). A strength of this approach is that it recognizes the middle of the life span as the most productive period, whether production is measured by economic output or by the amount of uncompensated care provided to dependents (children and elders). The approach is also qualitatively consistent with results from societal-choice surveys where citizens express a clear preference for saving life years in the middle of the lifespan compared to life years at

advanced ages (Cropper et al, 1994; Johannesson and Johannsson, 1997). Again, a limitation of this approach is that it is not grounded in the principle of consumer sovereignty.

In classical economics it is not common to compute estimates of benefits unless they are based strictly on the application of consumer sovereignty to populations at their current reference points with respect to baseline risk, age, gender, income, wealth and other personal characteristics. Yet EPA and other federal agencies have already departed from this strict position by refusing to allow variations in VSLs as a function of the income of the target population. Moreover, there is a significant literature in health economics suggesting that at least some of these reference points should not (normatively) be permitted to influence benefit calculations (Pratt and Zeckhauser, 1996). Social welfare functions that recognize equity concerns are becoming better accepted in the field of economics (Sen, 1979). Departures from valuation at current reference points have been justified on grounds of both efficiency and equity (Gold et al, 1996).

3. When a VSL derived from a healthy population is transferred to a population that will remain in a health state of diminished quality, a downward adjustment in the VSL should be made to account for the utility difference associated with living in good health versus living in poor health .

Discussion

It has already been established that willingness to pay for life years in old age is influenced by the quality of those life years (Johannesson M, Johannsson PO, 1996; 1997). The literature on QALYs (discussed below) provides a wealth of evidence on the utility differences between life years in good health and life years in various states of sickness and functional impairment. This body of theory and evidence should be acknowledged in EPA's guidance and used by agency analysts.

4. When VSLs are transferred to the environmental context, agency analysts should consider whether it is appropriate to make adjustments for psychological considerations (e.g., voluntary versus involuntary exposures) that are known to influence public valuation of risk reduction.

Discussion

There appeared to be consensus on this issue at the SAB meeting, at least as a vehicle to account for selection effects in the revealed preference studies involving workers.

5. When investments in environmental protection produce health gains only after a significant latency period (e.g., as is typical in the prevention of new cases of chronic disease such as cancer, heart disease, bronchitis, and chronic obstructive pulmonary disease), analysts should account for the latency period when time preference is handled in benefit calculations (Cropper and Portney, 1990). For prevention of acute health effects of pollution, such adjustments may not be required when the time interval between emissions and health effects is short in duration.

Discussion

There appeared to be consensus on this issue at the SAB meeting.

6. When valuing reductions in morbidity risk, agency analysts should consider the QALY approach and the available data for specific health effects that is published in the medical and public health literature (Torrance, 1986; Kaplan, 1995; Gold et al, 1996). In some circumstances, it may be feasible to derive rough estimates of willingness to pay for prevention of morbidity states using QALY values and VSL values.

Discussion

It will not be feasible to conduct an original morbidity valuation study for each benefits analysis that the agency is conducting. Yet many of the health effects of interest to the agency are the subject of extensive study in the clinical and public health literature using utility-based instruments. Results of these investigations can be transferred to willingness-to-pay values using approximate transfer functions (Tolley et al, 1994; Johnson et al, 1997). Although such approaches require some significant assumptions and may in some settings deviate from strict willingness-to-pay estimates (Pliskin et al, 1980; Pauly, 1995), they will offer agency analysts significant evidence and insight from the clinical and public health literatures. Moreover, the QALY approach was recommended for use in reference-case analyses by an expert panel of economists and decision scientists commissioned by the U.S. Public Health Service (Weinstein et al, 1996).

If these six suggestions are implemented, the agency will have taken a strong step toward harmonization of the fields of health economics and environmental economics. It does not make sense for different analytical communities to be valuing the same health risks with different approaches.

I have attached to this letter a list of references that I hope will be of use to agency analysts. I certainly am willing to provide further assistance and respond to any questions that may be raised about these important issues. Thank you again for the opportunity to participate in your deliberations.

Sincerely,
John D. Graham, Ph.D.
Professor and Director

cc: Dr. Albert McGartland
Dr. Tom Miller (SAB staff)
Dr. Milton C. Weinstein
Dr. James Hammitt
Dr. Richard Zeckhauser

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November 30, 1998

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Cambridge, Massachusetts 02138

Dear Dr. Stavins:

This letter provides an expansion of the remarks I made at the committee's meeting on November 18, 1998 on the review of the draft Guidelines for Economic Analysis.

My major concerns are the lack of discussion on uncertainty in the draft Guidelines, and, more generally, the lack of coordination between risk assessment and regulatory impact analysis within EPA. I was pleased that Dr. Paul Joskow had already pointed out the need for EPA to expand the discussion on uncertainty before I made my oral comments. This letter will provide an expanded discussion and some references that the committee may find useful.

Expanding the Discussion of Uncertainty Analysis

There is a brief discussion in Section 5.5 on “Analyzing and Presenting Uncertainty” (p. 22-24) and an even briefer discussion (9 lines) under the subhead, “Explicitly address uncertainty and non-monetized benefits” in Section 7.3 (p. 65). These discussions seem overly terse, given the importance of uncertainty, as acknowledged in numerous recent EPA documents. The discussions should be expanded at least to the order of 10 pages in EPA’s 200 page document. Specific guidance should be provided to explain where the uncertainties originate and how uncertainty should be handled in the analysis. A good, reasonably up-to-date general reference is the 1992 book, *Uncertainty: A Guide to Dealing with Uncertainty in Quantitative Risk and Policy Analysis*, by Granger Morgan and Max Henrion, which is briefly cited earlier in section 7.3, p. 65, on the need for scoping analyses. Much more useful guidance could be excerpted from this book. EPA should go even further in presenting methods from recent literature on economics, decision analysis, and risk analysis. Many excellent articles have appeared in *Risk Analysis: An International Journal* and the *Journal of Risk and Uncertainty*. The literature cited on uncertainty in EPA’s current draft is generally several decades old and is not well aligned with recent work in characterizing health and ecological impacts from environmental regulation.

The Integration of Benefit Cost Analysis and Risk Assessment

As someone who has come out of an educational background of operations research, decision analysis, and economics, and then participated on many committees of the EPA Science Advisory Board and the National Research Council dealing with environmental risk over the past twenty years, I am conscious of the gap between the environmental economics literature and the risk/toxicology literature in dealing with uncertainty. (One of my first National Research Council activities, in 1976, was serving on a subcommittee that Myrick Freeman chaired for a report on regulation of toxic chemicals.) I hope the current committee can work to close this gap. I welcome the opportunity to help.

The Mis-Read Book. I will recommend for the committee’s reflection and that of the EPA economics staff a number of reports from the National Research Council. The first is *Risk Assessment in the Federal Government: Managing the Process* (1983). This report was widely circulated inside and outside EPA, in large part because of references to this report by William Ruckelshaus, in his second term as EPA Administrator, regarding risk assessment and risk management. In my judgment, these references introduced a misunderstanding within EPA that persists to the present time. The popularity of this 1983 National Research Council report during the 1980s and early 1990s was reflected by its widely used nickname, the “Red Book.” This nickname came from the color of its cover and reference to the published sayings of a former leader of the People’s Republic of China.

Risk Assessment in the Federal Government: Managing the Process primarily addressed the assessment of cancer risk from environmental chemicals. An industry group, the American Industrial Health Council, had proposed the use of a science court mechanism for resolving risk assessment disputes. The National Research Council rejected this proposal and proposed instead a formalization of risk assessment through the use of guidelines, published documents, and peer review of these documents by scientists outside of the regulatory agencies. The report advocated a **conceptual distinction** between risk assessment, a process of summarizing science so as to inform regulatory decision makers, and risk management, a decision process involving not only science, but also the economic, political, and social consequences of regulatory decisions.

Mr. Ruckelshaus made several widely circulated speeches citing the “Red Book,” in which he advocated the *separation* of risk assessment and risk management. As a result the “Red Book” became the “Mis-Read book.” While the recommendations in the “Red Book” for development and adoption of risk assessment guidelines were adopted by the Executive Office of the President and subsequently by EPA, the admonition that risk assessors and risk managers should communicate early and often was lost. Also largely lost was the advice that the scope of the analysis should be tailored to match the needs of the risk managers. (The citation mentioned above to Morgan–Henrion (1992) on p. 65 of the Guidance draft on this very point needs more discussion to highlight the importance of the point!) EPA adopted a process of producing formal risk assessment documents as a prelude to environmental regulation. Moreover, risk assessments often took on a “one-size-fits-all” character. Uncertainty in the numerical estimates of risk was poorly communicated or neglected. Risk management decisions have been too often driven by single-number risk estimates and narrowly drawn distinctions in the weight-of-evidence criteria, such as a B2 versus a C categorization for carcinogens.

More Recent Reports. There are a number of more recent reports from the National Research Council that deal with uncertainty in environmental impacts from regulation. These include *Improving Risk Communication* (1989), *Issues in Risk Assessment* (1993), *Science and Judgment in Risk Assessment* (1994), and *Understanding Risk: Informing Decisions in a Democratic Society* (1996). I particularly recommend the last two as important in providing specific advice to EPA on risk analysis, and in correcting misinterpretations from the 1983 “Mis-Read Book.” Another important report is the report from the Presidential-Congressional Commission on Risk Assessment and Risk Management (1997). I enclose a paper summarizing a workshop for the Society of Toxicology, which describes main themes from several of these reports and recent documents from EPA. (“Risk Characterization: A Bridge to Informed Decision Making,” (with Edward V. Ohanian, John A. Moore, John R. Fowle III, Gilbert S. Omenn, Steven C. Lewis, George M. Gray, and D. Warner North), Workshop Overview, *Fundamental and Applied Toxicology* **39**, 81-88, 1997.)

Economic Analysis and Risk Analysis Serve the Same Purpose: Informing Policy Makers and the Public. The draft guidance document states at the beginning of Section 10.4, p. 197, that “The primary purpose of conducting economic analysis is to provide policy makers and others with detailed information on a wide variety of consequences of environmental policies. One important element these analyses have traditionally provided to the policy making process is estimates of social benefits and costs ...” Risk assessment, risk management, risk communication, and risk analysis (a term to encompass the first three) have the same purpose: to inform policy makers and others who have an interest in environmental policy decisions. This purpose is served only if the magnitude of the uncertainties on social benefits and costs is conveyed. “Detailed information on a wide variety of consequences” is not consistent with single-number, or point, estimates. The policy makers and others should receive a summary that discusses the origin of important uncertainties and the potential of further analysis and research to reduce these uncertainties, prior to policy implementation.

The Value of Information that can reduce uncertainty in the context of a policy decision is a very valuable insight from formal uncertainty analysis. I enclose a recent reprint of mine that discusses this concept qualitatively in the context of the upcoming EPA standard-setting process for arsenic. The importance of this concept is noted in both the 1996 *Understanding Risk* report and the Presidential/Congressional Risk Commission report.

I will welcome opportunities for further interaction with your committee, and you have my best wishes for success in helping to upgrade the quality of economic analysis within EPA.

Sincerely,
D. Warner North

cc: Tom Miller, DFO, SAB staff

Enclosures

ATTACHMENT H-1
Written Comments of EEAC Members

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Dr. Bockstael

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----- Forwarded Message Follows -----

From: "nancyb@arec.umd.edu" <nancyb@arec.umd.edu>

Subject: SAB meeting

Date: Tue, 17 Nov 1998 10:56:26 +0100

Rob -

Please don't feel you need to introduce all these points for discussion, by any means, but you may wish to turn these in to the EPA staff who are writing the guidelines for their consideration. While I haven't had time to read the entire document, this draft seems better than the first, but there are still some problems. Given what I perceive to be the importance of these Guidelines, it's probably worth the effort to get them into good shape. Here are some specific comments on Chapter 7. Some may seem nitpicking, but have more far reaching implications than may be obvious at this time.

Thanks. Nancy Bockstael

pg. 7-2. The statement is made that WTP and WTA will be close if there are small income effects. As Hanemann has shown, this is true for price changes, but the Willig results are not exactly applicable for quantity (or quality) changes. Even if income effects are small, if the good has no close substitutes, income flexibility can be large and differences can be large. Environmental quality changes generally take the form of quantity or quality and not price changes.

pg 7-2. The statements about consumer surplus are odd. No connection is made between CV/EV and CS. The authors seem to suggest that CS is an aggregate concept and one that applies only to market goods. Neither is necessarily true. CS is simply a "money held constant" rather than a "utility held constant" concept, and is the usual thing measured even in non-market contexts and for individuals.

pg.7-9. I have always had an unreasonable prejudice against the kind of pigeon-holing of "valuation approaches" listed in this table. It suggests that all revealed preference valuation approaches are idiosyncratic and stem from different underlying concepts of welfare. I won't argue strongly here, but I don't like it. It would be more effective to motivate the revealed preference methods from a household production perspective, then they don't look as though we're changing the concept of welfare to fit the problem.

pg 7-14. The authors argue that in the long run, producer surplus will disappear in competitive markets. This statement seems to suggest two things that are not really correct. The first is that most markets are perfectly competitive. The second is that there can be no rents or quasi-rents in competitive markets in the long run.

pg 7-17. The statement is made that discrete choice (or random utility models) are used when there are simultaneous environmental changes at a number of sites. While they can be used in this context, this is not the motivation for using them. The real reason to use random utility models is to get observations on behavior in the face of variation in environmental quality. Systems of demands don't typically work because site quality doesn't change over people at a site.

pg 7-18. While some of the confusion between marginal values and values for discrete changes has been cleared up in this draft, some still remains - particularly in the hedonic and averting behavior sections. For example, at the top of this page, to make it clear, this should read "reveal the marginal values they attach".

pg 7-21 through 7-22. The section on hedonics is still a bit shaky. The frequent reference to the second stage and the statement that "data limitations" prevent moving to the second stage are problematic. But, in fact, only if you have multiple market data or something of that sort is the "second stage estimation" meaningful in any way. The problem is not a data problem. The problem is that the hedonic reveals information only on the marginal valuation of anyone, and there is not way to identify anything else about individual's preferences for the environmental quality. (There is now much literature on this identification problem, but unfortunately the early literature was either wrong or misleading. It would be a shame to further propagate these errors.)

Pg 7-23 through 7-24. The same can be said about the averting behavior section, although here the problems are worse. The literature in this area has been very confused. The authors' discussion of the "full averting behavior model" suggests a misunderstanding. The sorts of terms present in the Harrington and Portney paper (pain and suffering, defensive expenditures, medical expenditures, lost wages, etc.) come from totally differentiating the LaGrangian (the individual's constrained utility maximization problem) with respect to the environmental quality. However, one can not just integrate each of these terms over the discrete change in quality to get a welfare measure. In addition, the authors suggest that this measure is the truth, while changes in averting behavior is something "less", but application of the envelope theorem illustrates that many of the terms in the H-P expression cancel out. My point here is that this section needs to be rewritten if it is to be this specific.

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Dr. Burtraw

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To: Robert Stavins
From: Dallas Burtraw
Comments on "Guidelines for Preparing Economic Analysis" (Nov. 3, 1998 draft)
November 23, 1998

Overall the "Guidelines" is a very nice document that promises to be a nice reference point for analysts. I focus my comments on areas for improvement, leaving aside the many opportunities for complements.

Chapter 5: Overview

This chapter is supposed to address cross-cutting issues. I feel the following issues should be addressed in this context.

1. Tax interaction effects

Evaluations of regulatory policy typically are set in a first-best regulatory setting. However, preexisting taxes such as taxes on labor income create a second-best setting. Recent advances in applied general equilibrium analysis have led to generally replicable results (at least in their qualitative nature) that indicate the direction of bias underestimating the cost of compliance, from a social perspective. However, this varies by regulation. However, a similar bias may exist with respect to benefits.

2. Baseline issues

The chapter, and indeed the entire document, does not provide guidance with respect to how forthcoming but as-of-yet not final regulations are to be treated in the baseline for evaluation of a specific policy. At the meeting of the EEAC I suggested that EPA practice was to only model regulations that were already final. One justification for doing so is to prevent abuse. However, I have been corrected, and informed that sometimes, forthcoming regulations are part of the baseline, which seems wise in most circumstances. In any case, there is no indication in the Guidelines as to standard practice.

3. Time

Time enters an analysis in several ways, but little attention is paid to any of them in the Guidelines. One way that time matters is in the pace of exogenous technological change. In principal this can either increase or decrease marginal and total abatement costs, depending on the direction of change, but in general we would expect it to decrease abatement costs. Recent analyses have indicated the magnitude, even for mature technologies, can be striking. See: Ellerman (recent Energy Policy) and Carlson (http://www.rff.org/disc_papers/PDF_files/9844.pdf) for studies pertaining to changes at coal-fired power plants.

A second way time matters is innovation induced by a regulation. Chapter 5 would seem like a good place to review the literature on regulatory incentives for innovation.

Chapter 6: Social Discounting

While I remain somewhat agnostic on this controversial area, the authors may want to see the recent article by Marty Weitzman in the most recent JEEM for evidence on behalf of the argument put forward in the chapter.

Chapter 7: Analyzing Benefits

1. Coordination

Although I refrain from drawing attention to good passages in general, I must draw attention to lines 18-24. This call for coordination is extremely important. Too often legislators have stated that recent analysis fails to address options that are being considered actively.

2. Mortality Valuation

At the EEAC meeting I argued that adjustments of the nature introduced by John Graham are desirable. However, I went on to state evidence supports Al McGartland's contention that

individuals do not value their remaining year(s) of life in a manner that is independent of their reference point. (This is true independent of changes in wealth that occur over a lifetime.) In other words, I disagreed with the suggestion that an "adding-up condition" would apply and each year of longevity should be treated equally. I invoked consumer sovereignty as justification for taking WTP of a 69 year old for one year life extension at "face value."

The overarching point is that valuation of lost life years should depend on the context of the injury. Imagine the case of a one-year extension in life, conditional on achieving 69 years of age, stemming from reduction in chronic exposure to particulates. In this case, neither the application of a VOSL derived from a prime age male's workplace choices, or the VOSL of a 69 year old for a one year life extension, reflect the proper context for opportunity costs that should be considered in the valuation. Assuming the chronic nature of the injury, to avoid the injury would incur costs over a 70 year lifetime. Each year, the value that an individual would be willing to pay could be presumed to vary. This argues against using a single number in the first place. But if a single number is all we have, then adjustments are desirable, but they should not be applied in a linear fashion.

Three studies come immediately to mind to make the point that the WTP for a life-year can be expected to vary with age.

- (1) Jones-Lee illustrates that there is an age-sensitivity that should be taken into account, but clearly it is not proportional to expected longevity.
- (2) Johannesson and Johannsson (1997) point out that that the expected quality of life in old age is much less than the value that is assigned by people in old age.
- (3) Murray and Lopez (1996) (from public health) offer another example in which the value of a year of life (holding health status otherwise constant) is not treated constant.

In summary, I agree with the implication of John Graham's presentation that adjustments are important and desirable, but I argue they should not be applied in a linear fashion.

Furthermore, as Graham and Richard Revesz argued, there are justifications for adjustments that head in the positive direction. Leading among these is the involuntariness of environmental risks.

Finally, let me highlight one realm for disagreement. Cropper et al (1994) that individuals would prefer to allocate resources to saving lives of the young at a ratio *greater* than the proportion of life-years remaining. This is interesting, but as long as benefit-cost theory adheres to consumer sovereignty in a rigorous manner, the WTP of individuals at a given reference point is the determinant factor. Hence, though environmental economics may have some catching up to do with public health, the distinction between allocating resources in a public health context and the practice of benefit-cost analysis in environmental management is a meaningful one and we should not expect it to disappear.

Chapter 8: Analyzing Social Costs

1. Partial equilibrium analysis

The presumption here is that partial equilibrium analysis is acceptable, and perhaps preferable, given resource constraints. I suggest the writing be a bit more equivocal about this.

The main problem I had with the text in this regard was the attempt to establish equivalency between compliance costs and social costs. The two may be proximate, but they are never synonyms. The text should be cleaned for where the terms are used interchangeably.

Second, the text suggests that the greater the opportunity for substitution the lower would be social costs compared to compliance costs. One reason this may not be true has to do with the type of policy being implemented. A technology standard does not allow for the same opportunity for input substitution as does an emission rate standard (Page 8-9, lines 1-10). Also, there is reason to think things might go in this direction, but they could well go in the opposite direction. This is likely to vary over time.

2. Time

Hazilla and Kopp (JPE) and papers by Jorgenson have established the time profile of social costs. In Hazilla and Kopp, social costs were less than compliance costs due to the opportunity to substitute among inputs in production. However, over a longer time horizon it was recognized that environmental compliance diverted investment capital from its highest valued use and set the economy on a different growth projectory that magnified social costs over time.

A second way that time matters is through innovation and dynamic efficiency. In this section there should be some discussion about innovation and how it might be expected to affect social costs (this should be foreshadowed in Chpt 5, as I suggest above).

3. Tax interaction effects

Tax interaction effects offer a reason why social costs may be systematically greater than compliance costs. In particular, the last sentence of the conclusion should be revised. See also pages 8-14 (CGE models are relevant even in the absence of a double dividend. See Parry, JEEM, 1995), 8-17 (where the discussion of marketable permits at lines 5-10 dismiss social costs – see Goulder, RAND Journal, 1996) and 8-21 (last sentence).

4. Provision of compensation

Compensation can affect avoidance behavior by creating adverse selection and/or moral hazard problems. This is a central idea of Coase. Text should be addressed at page 8-20.

5. Minor edits:

Page 8-3, line 19: Insert word “marginal”
Section 8.4.1 The terminology in this section is unfortunate. 8.4.1.1 is labeled “Performance Standards” but actually seems to describe “technology standards.” Consider BAT and BPT. A performance standard is a term that fits emission rate standards, where there is some meaningful choice among investments that might be made to achieve that rate.

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Dr. Cameron

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TO: Robert Stavins
FROM: Trudy Ann Cameron
RE: detailed comments on "Chapter 7: Analyzing Benefits"
DATE: November 20, 1998

I will go line-by-line through my marginalia on the draft of the Benefits chapter, making suggestions as I go. The minutiae will be included as well. While I would like to devote more time to this, there is a binding constraint. I think this hits all the high points. Where some of my concerns overlap substantially with those of Nancy Bockstael, I forgo pointing them out again, since her comments will presumably be part of the record.

p. 1, line 45: "reveal" not "revel"

p. 2, line 21-23: WTP may or may not be easier to measure and quantify. I have most often heard it advocated as being "a conservative measure of value" rather than "the right measure of value." Since it is almost always less than WTA, it is more palatable to people who would prefer that the benefits be small.

p. 2, line 35-38: consumer surplus can be estimated from individual fitted demand curves as well as from aggregate market demand curves. This needs to be acknowledged at the beginning, although it is probably OK to emphasize value estimates from overall market demand curves, since these are often used. In line 37, it should probably say "how much of the good is demanded in the aggregate at each price".

p. 3, lines 10-13: this gives the impression that elasticities are sufficient to derive changes in surpluses. Only constant elasticity demand curves (e.g. log-log models) have just one elasticity that is relevant everywhere. It would be more appropriate to acknowledge that you need to know the configuration of the demand (or supply) function in the relevant region, not just some summary statistic such as demand (or supply) elasticity.

However, it is important to remind non-specialists that a common naïve assumption is to think that quantities traded will be unaffected by price changes (i.e. zero elasticity). This may not be valid.

IN GENERAL: There needs to be greater priority associated with benefit transfer, what it is, how it should be done. This is likely to be the main way any of this gets implemented, and it will be more valuable to inform the consumers of this document about how to choose between existing studies as candidates for benefits transfer than to teach them how to conduct their own original studies.

p. 7, line 32: explain what a "cost analyst" is. I'm not sure, for one.

Footnote 5: Car[c]inogen Risk Assessment

p. 8, line 40 "Guiding principle[s]..."

p. 8, lines 47-49: Awkward to read. Perhaps "This section describes the [categories] of benefits that are typically associated with environmental policies. [These descriptions are provided with understanding that quantification of benefits in each category will typically be desirable and should often be attempted.

However, the different possible techniques for accomplishing the quantification of the values of these benefits will be reserved for a later section.]”

Exhibit 7-1: It is risky to compartmentalize the different valuation methods as appropriate for different types of services flows. There is a lot of redundancy across benefit categories in the Human Health part. Non-market methods (e.g. stated preference) can supplement market methods for ecological benefits, to extend the domain of demand functions beyond the range observed in the historical data.

p. 11, line 15: “characterizations of a behavioral response to illness.” Perhaps there is some merit in commenting upon the use of subjective responses as opposed to objectively measured effects. Some readers might comprehend the idea that these characteristics of morbidity are themselves endogenous, they do not constitute exogenous shifters of willingness to pay for mitigation.

p. 11, line 46: I suspect it is Crock[er] and Shogren (1991).

p. 12, line 11: “..three steps ...[were] discussed...”

p. 12, line 19: “...focusing on those service flows [that/which?] are likely...” (I can never remember the right usage.)

p. 12, line 23: “Not only [is it] useful as a conceptual...”

p. 12, line 38-39: This is awkward. If the idea is “rivalry in consumption ,” perhaps that could be used explicitly. Anybody with Econ 1 should have heard about the problems with public goods. What is meant by “..benefits received by any one individual tend to affect benefits accruing to others.” In what way? Decreasing them? Enhancing them?

p. 13, line 7: Define the concept of an “instrumental” benefit. This is the first encounter with the term in the document. Perhaps the next sentence defines the term, but this role of that sentence is not made clear.

p. 13, line 21: can be expected to include [the] magnitude of [the] effect (and so on). Or, use magnitude-of-effect as one “thing.”

p. 13; line 38: elaborate a little on what is intended by “household soiling.” Dirty paint? Dirty roof shingles? Dust tracked into houses?

p. 14, line 34-37: the discussion of profits being competed away is just too pat. When have we ever seen a perfectly competitive industry? (If you stick with the current wording, line 36 should refer to a perfectly competitive [industry] (not a “model”).)

p. 14; line 44: “flushed out” seems a n incongruous term to use here.

p. 14, line 46: Mention what a “damage function approach” might be....

p. 14, line 48: Mention elasticities, cross-price elasticities, and substitution possibilities. This came up earlier, and might be referred-back-to.

p. 15, line 9: “Factors that influence the complexity...” In this context, the reader has a good chance of thinking that you are referring to factors of production, the previous use of the term “factors.”

p. 15, line 10: hyphenate “single-product” and “multi-product”

p. 15, lines 12-18: Make it clear whether you are talking about an increase or a decrease in environmental quality. This passage will be a little hard to follow for the uninitiated. Line 16: [other] factor prices?

p. 15, line 32: selection of [a] modeling approach

p. 15, line 35: Is it necessary to point out that “More-extensive treatment of consumer and producer responses is preferred to less.” Maybe that is not obvious, I suppose.

GENERAL: all kinds of valuation methods have systematic and familiar problems. For some reason, the longevity of a method seems lead people to judge it as more reliable, when this is not necessarily the case. Adequate caveats about the existence of a whole range of perils in all of the methods (not just CV) would be appropriate.

p. 16, line 22: Some recreation demand models are suited to new site assessment (notably a RUM model with McFadden’s conditional logit format and no simply respondent-specific explanatory variables). Others, like the travel cost model without any hedonic dimension, are less appropriate. They can handle decreases in access to existing sites, and might be stretched to predict increases in trips by current users, but cannot handle recruitment of entirely new users or be transferred to different sites with a different set of characteristics.

p. 16, line 26-27: There is some incongruity in this sentence The issue of “target species” crops up even though fishing or hunting have not been mentioned explicitly before. Not all recreation involves bagging wildlife. Also, is it intended to say “difficult [for the researcher] to observe” or “difficult [for the subject] to observe. It matters.

p. 16, line 35: and the [relationship among the variables] may be [interpreted] as a demand function

p. 17, line 11: There is an appropriate citation for this. I will try to find it. Kealy and ?

p. 18, line 26: why the “speed” of the work is relevant needs to be explained a little...

p. 18, line 31: workers’ perception[s]

p. 18, line 32 the resulting estimates of [the] compensation. There should be some comment about why workers generally underestimate on-the-job risks. This could be something about perception that would apply to any randomly selected individual. However, it is often the case that individuals self-select into risky jobs based on the disutility they experience from exposure to these risks. For example, many chemists swear “chemicals” are not bad for you, when ordinary people would be much more fearful of “chemicals.”

Footnote 8. Explain repeat sales analysis, however briefly.

p. 19, line 16: is this intended to say that there are estimated risk values for a variety of specific types of risk? Right now it is ambiguous, in that it may seem that the risk values are substantial.

p. 19, line 25-26: How is it that the injuries are often not defined specifically enough? Is a broken arm not a broken arm? An example of a typical ambiguity would help here, since the issue has been brought up.

p. 19, line 31: when mentioning the “number of difficulties” include parenthetical reference to one or two of them. Are these things like colinearity problems? To what does this mention refer?

p. 19, line 43: give an example of an occupational disease.

p. 20, line 1: can this simply be identified as a “self-selection bias” problem

p. 20, line 9-14: actuarial data to determine the risk levels faced by workers [in different occupations]. Elaborate on the idea of a fatality risk “off the job.” Would these be constant across workers, or correlated with occupation?

p. 20, line 45: does not match individuals’ perceptions, then the results of the analysis [may] be biased.

Be consistent: “a hedonic” or “an hedonic”

p. 21, line 11: Studies have used data on transaction involving both structures and land exclusively. This is ambiguous. How can you use both, exclusively? This seems to contradict.

p. 21, line 19: The information and modeling requirements of these states [differ] tremendously.. When you get to “data limitations often preclude the second stage of analysis,” I would have expected a little more discussion. For one thing, demand functions correspond to individuals, not to houses, so we need to gather additional data on buyers of houses, including their incomes and the choice set they faced when they made the decision. There are all kinds of thorny problems in the process of getting from an hedonic price function to anything that looks enough like a demand function to allow for welfare calculations.

Footnote 11: For example, “this holds” if few properties... What holds? Also, what is an equilibrium price function? Presumably, it is some reduced form of the market demand and supply functions. This makes it sound like something specific and different from the “hedonic price function.”

p. 22, line 4-8: This is awfully vague and sketchy. (first, line 5 is missing ...[the] analysis). The data limitations that are described seem only to be errors in measurement. These things seem advertised to affect only the second stage of the modeling process.

p. 22, line 20-22: One cannot overlook the possibility of lost “interim” value, even if snapshots of a housing market in equilibrium prior to some environmental insult or regulation, and sufficiently long after the incident or regime change, show relatively little loss of value. We don’t want to be misled by variations over time in value, but we do not want to discount these completely, either.

p. 22, line 30: Rather than using the potentially technical term “limited priors” (in the Bayesian statistical sense) I would suggest “little guidance.”

p. 22, line 39: Mention what “market segmentation” is.

p. 22, line 41-42: If you are not going to explain the intuitive rationale for the bias and efficiency results, provide a citation for these findings.

Footnote 12. Are fecal coliform counts observable to the consumer? I.e. can you see them in the water? Or just smell them at sufficient concentrations? Any more than any other water content data.

p. 23, line 1-3. Beware of advocating R-squared as the objective function in fitting an hedonic price function. “Stable” results in what sense? Derivatives that are robust across alternative specifications?

p. 23, line 9: “...included in this set.” Which set?

p. 23, line 22: What is meant by “a continuous relationship”? ...small reduction in health[,] or health risk,...

p. 23, line 35: ...lower bound [on] willingness to pay.

p. 23, line 39: If estimating [a] full averting behavior model...

Footnote 13: in what sense is the impact of changes in risk on self-protection expenditures “ambiguous”? (This may just be an awkward exposition.) Insignificantly different from zero, or significantly different from zero in opposite directions under different specifications with the same data.

p. 24, line 23: “reduced soiling of materials” perhaps some amplification at the time of the prior reference to this effect would save people from wondering what this is. What materials? How?

p. 24, line 29: I wonder if this means that we simply observe whether or not people are willing to incur costs at different levels to reduce risk. This makes it sound a lot like the same empirical environment as referendum CV analysis. Could you not infer WTP by observing the binary choices of a lot of people with similar preferences, for different levels of averting cost?

p. 24, line 41: ...Averting Behavior_ Studies

p. 25, line 10: Separability [from] other benefits? ...Are there likely to be instances when mitigation involves other related costs, rather than other related benefits?

p. 25, line 24: “simply summing the realized costs...” By this, do you mean “explicit market” costs, as opposed to nonmarket or implicit costs?

p. 25, line 30: It seems a bit oblique to say that the “theoretical basis for the cost-of-illness method is very limited.” The two assumptions mentioned may be “broadly consistent with neoclassical economics,” but are they anywhere near “broadly consistent with the real world”?

p. 25, line 38-39: altogether, not all together

p. 25, lines 49-50: ...this difference may vary greatly across health effects [and across individuals]. Understanding WTP requires knowing quite a lot about an individual’s preferences, which can vary dramatically from individual to individual.

p. 26, line 1-3: If you are going to introduce the distinctions among ways of valuing lost work hours, there needs to be a rationale.

p. 26, line 6: panel of physicians to develop a [generic] treatment profile

p. 26, line 8-9: data availability (data on what?)

p. 26, line 17: ...and data are often readily available... This appears to somewhat contradict the assertion above that data availability is often a determinant of what approach is used.

p. 26, line 24: by “expected averting costs,” do you imply statistical expectations, or just “typical” averting costs.

p. 26, line 26-27: seems redundant with footnote 14, page 24.

p. 26, line 31-32: Although the approach [may] account_ for costs... ... or [any costs that may have been incurred in order to avoid] the illness.

p. 27, line 1-3: It would help to emphasize that the method is NOT individual specific. The fact that the “wage rate chosen should reflect the demographic distribution of the illness under study” is less confusing when readers can remember that this is not individual data.

p. 27, line 6: Lost value of leisure time... (To whom, the individual who is sick, and/or their families? Other’s may value the individual’s leisure time as well.)

p. 27, line 17. Use “likely choices in a hypothetical market” rather than “preferences for a hypothetical good.”

p. 27, line 35: bias can be introduced easily into these studies if they are not carefully done (or even if they are!)

p. 27, line 41: Surveys without these tests [should be suspect;] surveys whose results fail these tests [may be] discredited.

p. 27, line 45: make choices between two [or more] (CA)....

p. 27, line 49: Multiple choice conjoint analysis is a take-it-or-leave it choice as well. There are simply more alternatives to the “status quo” choice. Since the information presented to the respondent is richer, their choices under these circumstances contain more information. ... “presented in the latter” is confusing. Which method is the “latter”?

p. 28, line 18-19: Since you bring this up.... It is not valid simply to assume that the nonuse values enjoyed by current users are equal to the total willingness to pay of nonusers. The reason, again, is sample selectivity. People who choose to be users may have fundamentally higher values for the resource than definite non-users. If I may be so bold, T.A. Cameron and J. Englin (RAND Journal of Economics, volume 28, no. 0, Special Issue ’97, p. S45-S70) makes this point explicitly. Paper title is “Welfare Effects of Changes in Environmental Quality Under Individual Uncertainty About Use”

Another relevant paper is "Respondent Experience and Contingent Valuation of Environmental Goods" (Trudy Ann Cameron and Jeffrey Englin) *Journal of Environmental Economics and Management* 33(3), 1997, 296-313. In this, we show that prior experience with a good (including "none" implying non-user status) has a statistically significant effect not only on expected WTP for an environmental good, but also on the noise in estimated values (furthermore, experience and values are modeled as jointly endogenous variables).

p. 28, line 25-26: I am always troubled by the assertion that "WTA applications of CV are much more problematic because, unlike the case of WTP, there is no budget constraint." This is technically incorrect. There is a budget constraint, there is simply no upper limit on the size of the opportunity set that defines compensating variation, whereas current income is an effective upper limit on the equivalent variation that would correspond to the same loss of environmental services. Differences between WTA and WTP are an artifact of people's preferences (the shapes of their indifference curves). It is perfectly possible that people will have indifference curves that are so far from "vertically parallel" that WTA will be vastly larger than WTP. Preferences are what they are, and consumer sovereignty demands that we respect these preferences. Where the problem lies is typically in the potential for strategic overstatement of WTA. The elicitation method can sometimes be faulted, but not people's preferences.

p. 28, line 31-34: "...and the problem that many respondents display intransitive preferences over the numerous, and often complex, set[s] of choices." Fortunately, myself, a student (and a colleague) are all working on these very issues. Namely choice set complexity and its effects on the preferences that are implied by CA studies. Much progress has been made over the last couple of years in the assessment of the effects of complexity on the error dispersion in random utility models of this type. Heteroscedasticity is a widespread problem, as a result of this complexity.

p. 28, line 36-39: it would be helpful to review the ordering of mail, telephone and in-person surveys by cost-per-observation and by their typical shortcomings.

IN GENERAL: It would be appropriate to note that contingent valuation has been very controversial in the past, in part because of its relevance to some extremely high-profile legal cases in the environmental valuation context. It is perceived as being more problematic than other methods of non-market valuation, but one must remember that the older methods are also subject to serious problems when badly applied. Alan Randall had a nice paper a few years back detailing the shortcomings of the travel cost method, for example. In the same context, however, readers must be reminded that despite the discomfort of many traditional economists with stated preference data, it is the only game in town in many contexts (especially for exclusively non-use values).

p. 29, line 14: Theory never tells us which variables will be "significant." It tells us which variables are likely to be important determinants; we merely hope that the coefficients on these variables will be statistically significant in our empirical models.

p. 30, line 22: would reduce the uncertainty of the [current] benefits estimate.

p. 30, line 27: well[-]accepted steps

p. 30, line 50: "the baseline criteria" What is a "criterion" in this context???

p. 31, line 10 (and elsewhere). I think "judgment" has only one "e"

p. 31, line 17 “...allows the analysis to [identify systematic] variation in existing...”

p. 31, line 43: fatal [risks] have been valued

p. 32, line 38-40: Is this a blessing upon the assumption that preferences do not change, or that we have identified all relevant heterogeneity in risk preferences across different groups? Noting that there seems to be little interest in developing new wage-risk studies may imply satisfaction with currently available numbers, for all purposes. This may not be your intent.

p. 33, line 8-9: [In practice,] there is a lack of a clear consensus...

GENERAL: section 7.6.1.3 may convey the misleading impression that these 26 particular point estimates are “gospel” on the issue of value of life. In fact, there is no presumption that these 26 values are independent random draws from some common underlying distribution of values of life, so that taking the average (in any sense) will provide an unbiased estimate of the true but unknown underlying mean value of life across all situations. This is simply a measure of central tendency among 26 separate estimates of the value of life derived from a wide variety of different studies. No mention is made of weighting each of these point estimates by the inverse standard errors associated with them (for example) by the original authors. Noticing that a Weibull distribution happens to provide an apparently good fit to the simple “marginal” distribution of these 26 values is rather shaky statistical justification for using the mean of \$48 million for the “true” value of a statistical life.

At a minimum, one might contemplate using these 26 numbers in a regression meta-analysis, of the type used by Smith and Huang for hedonic air quality studies. Interesting explanatory variables for the point estimates that come from each study would include the method and the vintage of the study, for example (data that are available in the tabular presentation of these 26 values). I am sure there are more dimensions of heterogeneity in conditions across these 26 studies that might make a difference to the size of the point estimate that results. There will especially be differences in the precision of the value estimates (trusting, of course, that information on precision was reported in the original studies). Conditional means, for studies under different conditions, might be more informative for benefits transfer exercises.

It is somewhat troubling that in all other benefits estimation exercises, consumer sovereignty is paramount, but in the case of value of life, we bow to political correctness in refusing to allow the value of life to differ systematically with sociodemographic attributes of the group in question. I agree entirely that it would be uncomfortable to admit that the social value of persons of different gender or ethnicity may differ. For some reason, only systematic differences by age appear to be admissible, despite anti-age-discrimination legislation, followed by health status and perhaps income.

p. 34, lines 38-40: Some mention should be made of the obvious arbitrariness of “using 75 percent of the VSL value for incidence among individuals over 65 years of age” Otherwise, this may appear prescriptive.

p. 38, line 41: is this double-counting, or simply over-estimation?

p. 39, line 16: “Potential shortcomings should be noted [even] for those studies that are considered to be of sufficient quality...”

p. 39, lines 27-31: It should be noted that one should be careful to assess whether the benchmark studies used to bound plausible estimates are themselves biased.

p. 40, lines 10-12: Ambiguity would be resolved by using: “they would value change in health status [that are certain.]” and “when using values for health effects [under certainty] to assess the changes in risks...”

p. 40, lines 37-42: “[A]lternative approaches..... attention of late[. However,] the results of these studies..... not well grounded in economic theory[, nor are they] typically applicable to policy analysis.

p. 40, line 45: “the broad range of service[s]”

p. 41, line 41, line 49 Explain the acronyms “O&M” and “POTW”

ATTACHMENT H-2
Written Comments of EEAC Members

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Dr. Kling

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From: Cathy Kling <clk@headyhall1.econ.iastate.edu>
To: TOM MILLER <MILLER.TOM@epamail.epa.gov>
Date: 11/29/98 8:39am
Subject: guidelines for economic analysis

Tom, My only additional comment on the guidelines for economic analysis is that once the document is complete, it might be useful to consider providing an index.

Thanks, Cathy

Catherine L. Kling
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Dr. Sigman

*

To: Tom Miller
From: Hilary Sigman
Re: Comments on draft "Guidelines for Preparing Economic Analysis"

I generally admire the document, which handles concisely and clearly many complex issues. Some specific suggestions are below.

Comments on chapter 6:

1. My sense is that there is more support in the economics literature for using conventional rates in intergenerational analyses than the chapter indicates. The chapter seems to draw too stark a distinction

between the two cases. The logic of potential Pareto improvements accepted earlier might apply here and is dismissed more readily than a reading of the literature would suggest.

2. The final choice of a .5-3% range for the intergenerational discount rate appears to come from the IPCC report. It might be appropriate to acknowledge the source, since it provides more background on the derivation of these numbers.

3. The chapter occasionally suggests that uncertainty can provide a rationale for not worrying about discounting. But, even the most uncertain benefits and costs should be discounted because they may otherwise give a more misleading impression. Places where the text gives a counter impression:

(1) page 6-1 lines 35-38;

(2) page 6-27, lines 33-47.

4. On page 6-15, the discussion might make clearer that the relevant tax rates for the wedge between the two rates of return is the effective marginal tax rate and that it is somewhat difficult to determine this rate in aggregate.

5. Page 6-4, lines 28-31 suggests an approach that would avoid discounting benefits. This approach would be inappropriate if the benefits vary over time. The approach is explained in a way that makes this problem more clear on page 6-28. I would keep the latter discussion, but skip the former.

6. Typo: p. 6-6, line 21, "to."

Comments on chapter 9:

1. I think there is too much emphasis on market power (section 9.2.6.1.1 and 9.2.6.1.2). The current discussion seems to require a very wide ranging and in depth analysis of complex conditions as a starting point for *every* analysis. Market structure issues deserve mention, but they seem much less useful than supply and demand elasticities and that prioritization does not come out clearly in this document. The chapter should suggest more of screening procedure in which a detailed analysis of market power is not recommended unless there is good reason to think that market power is important and its implications can be well understood.

2. On page 9-16 lines 12-15, the chapter asserts that firms with greater market power will be able to pass through more costs to consumers. In fact, that's not clearly true. For example, a monopoly will not entirely pass through an increase in marginal cost to consumers, whereas a perfectly competitive industry will with perfectly elastic supply.

3. The discussion of impacts of compliance costs on prices (section 9.2.7) should distinguish between costs that affect firms' marginal costs and those that do not. Only the former can be passed through to consumers (unless there are some more complicated effects through the number of firms in the industry).

4. This chapter (or the previous one) might provide some guidance on estimating supply and demand elasticities econometrically from price and quantity data. It worries me that the specific methods discussed for finding elasticities are non-econometric (such as section 9.2.7.3), when a good econometric method would usually be preferable. In addition, it might be useful to clarify that a two-equation method (e.g., two-stage least squares) is necessary to identify elasticities correctly.

5. In equity analyses, the chapter considers only the comparison of annual income to the poverty line as a way of identifying low-income populations. We might also wish to consider measures of lifetime income. One quick way to do this is to look at low consumption groups, since consumption will track households' expected lifetime incomes better than annual income. For example, one can divide the population by consumption deciles and see how the lowest deciles fare. It will not always be possible to do these analyses (for example, the Census does not contain consumption data), but if one were to look at the incidence of an energy tax, for example, this approach might be useful.

6. I disagree with using physical sensitivity as an equity dimension (page 9-47, lines 9-13). It is obviously important in risk analyses. But unless we have some separate reason for thinking we should ordinarily treat these people as deserving extra weight, there is no reason to do so now. (People with asthma sound special, but what about smokers?)

7. Why treat high property tax jurisdictions as weak financially (page 9-35)? Perhaps they just desire a high level of services.

8. The chapter asserts that strong growth increases ability to pass through costs (p. 9-15). It is not clear that this statement is true: it depends on the elasticities.

9. Typo: p. 9-8, line 14, "The is."

Comments on other chapters:

1. Section 4.4.1, p. 4-2 line 44: Deposit-refunds are more like taxes than "specialized forms of subsidies." Their net effect is equivalent to a tax on items that are not returned for the refund.

2. Section 5.3.2: I was confused by the inclusion of compliance in the section on "baseline specification." I think this discussion is excellent, but would have considered it to belong under the section on "predicting responses to a new environmental policy."

3. Section 8.2.4: Many of the "temporary" effects, such as plant closures and resource shifts to other markets, can also be permanent.

4. Section 8.3.5.1: The chapter might make the recommended use of input-output methods clearer. Conventional input-output analysis is helpful in analyzing the distribution of costs (e.g., the analyses in chapter 9), but it is not clear to me that it is especially useful in evaluating the level of costs, unless combined with some of the other forms of analysis discussed here.

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Dr. Viscusi

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From: Kip Viscusi

To: DCFCH01.DCFCHPO1(MILLER-TOM)

Date: 11/23/98 4:14pm
Subject: Economic SAB Comments on Guidelines

Dear Tom:

Below are my comments on Chapter 7 of the "Guidelines for Preparing Economic Analysis" document. The only comments that I wish to make for the record are those pertaining to Chapter 7. I will do so roughly in the order in which the comments appear.

First, let me make note of a point that came up at the meeting in relation to Chapter 7, but I believe belongs in Chapter 6. The role of the latency period is really a topic that pertains to discounting. Since the health impacts involving a latency period do not occur until after the period of exposure, the appropriate time frame for discounting should be the time in which the benefits accrue, which is the time which the diseases are prevented, not the time of exposure. This should be made explicit in the discounting chapter. Now let me turn to Chapter 7 where, unlike my comments at the meeting, I will present them in the order in which they occurred in the chapter not in the order of importance.

Page 7-6

In addition to quantifying the physical effects, in the case of health effects it is important to quantify the age distribution of the effects. In particular, if there are lifesaving activities, what is the age and the current life expectancy of those whose risks will be reduced? Recognizing these influences is important due to the great variations in life expectancy loss based on the cause of death. From an economic standpoint, the appropriate procedure is to examine the discounted expected life expectancy effects, where the role of discounting in this case makes the role of such quantity adjustments less influential than if no discounting were undertaken. Such estimates for a wide variety of causes of death appear in W. Kip Viscusi, Jahn Hakes, and Alan Carlin, "Measures of Mortality Risks," *Journal of Risk and Uncertainty*, Vol. 14, No. 3, (May/June 1997) pp. 213-233.

Page 7-9

Here, as well as below, when you discuss morbidity risks, the cost of illness approach will be mentioned. This is very much a second best type of approach that is only worthwhile when good willingness to pay data are not available. Cost of illness measures only provide a lower bound on the benefits, and often not a very good lower bound. It should be noted that until 1982 when I got the Occupational Safety and Health Administration to switch to the value of life approach that formerly federal agencies valued death by the "cost of death," which was similar in that only the present value of lost earnings and actual medical expenses counted. Doing so undervalued life by a factor of 10. The same could be true of some illnesses that go beyond coughs and colds.

Page 7-11

Near the end of the page "Crock and Shogren" presumably is not an editorial statement on Tom Crocker's work.

Page 7-12

I don't believe that indirect benefits should be counted at all. If you were going to count such indirect benefits, then one also has to account for the indirect costs, in particular the contribution of the opportunity costs "to the provision of another service flow." Unless, calculating indirect

benefits is narrowly defined in a way that avoids an asymmetry in how you count benefits and costs, I don't see how you can justify including it.

Page 7-13

I thought the wording of "an individual's commitment to environmental stewardship" was noteworthy since it is often claimed that contingent valuation surveys give upwardly biased answers because they are just eliciting such a warm glow benefit value. In any event, at this point contingent valuation was very much on my mind, but this chapter says next to nothing about what is clearly a central benefit valuation technique for environmental effects. Why?

Page 7-18

I thought that these caveats with respect to worker data were overdrawn since a number of studies have analyzed quite specifically the role of worker risk perceptions. See W. Kip Viscusi, "Employment Hazards: An Investigation of Market Performance," (Cambridge: Harvard University Press, 1979) as well as the Viscusi and O'Connor article that you cite. More importantly, this document is very strong on the caveats with respect to the wage studies, which are far and away the best studies, while at the same time even more caveats are pertinent to the other kinds of market evidence that have been used to assess the value of life. One would get the impression from reading this report that wage studies have the most drawbacks, not the fewest.

Page 7-19

In terms of applying the benefits of the wage studies it is important to note what they pertain to. This is perhaps my most important comment on the chapter. First, wage studies involve acute accidental deaths, not cancer. Although it is noted on page 7-19 that occupational diseases are not included, we do not know that this causes any bias in the risk tradeoffs for acute injuries. The real issue then is whether the premiums for accidental deaths are the same as for the kinds of deaths prevented by EPA policy, such as cancer. Consider, for example, the results in my survey, W. Kip Viscusi, "The Value of Risk to Life and Health," *Journal of Economic Literature*, Vol. XXXI, (December 1993), pp. 1912-1946. In Table 7 we review the valuations for fatal lymphoma based on Viscusi and Huber (1991) and find has a \$4 million dollar value that is roughly comparable to the value for acute accidents * certainly within the bounds of error. Although EPA can and should refine over time the estimates of the benefit values for different causes of death, it does not seem that there is great variation due to this influence.

There is, however, an important variation with respect to duration of life lost. My series of papers with Mike Moore indicate that this factor is quite influential. How one should handle this as a practical matter is clearly an important benefit concern because the people at risk in the wage-job risk studies have an average age in their 30s with much more life to lose than many of the target groups exposed to environmental risks. Until more precise estimates are available of the differential value of life at different ages, I would propose that all ages be treated symmetrically in terms of the weight placed per discounted year of life expectancy. EPA reports should consequently show two sets of numbers - one based on a cost per life saved without any adjustments for age and a second estimate based on a normalized life saved using discounted life years for the normalization. Equivalently, it could be in terms of discounted life years saved.

Page 7-21

The number of analytical bridges that have to be used in property value studies to estimate values of life are enormous. Also, if it is assumed that workers have trouble figuring out job risks, how well do people do in terms of identifying sources of pollution and the effects of pollution on their lives? I would think that this is much tougher. Moreover, the only study in which direct estimates of the risk appear in a property value equation * as opposed to some other measure such as pollution levels * to the best of my knowledge is the unpublished work that I have done with Ted Gayer and Jay Hamilton, which we are not yet circulating. As an aside, I would have put the automobile price literature ahead of the property value literature in terms of there being better estimates. The previous studies in the literature are reviewed in my JEL piece as well as in Fatal Tradeoffs. The more recent, and the most comprehensive valuation estimate appears in Mark Dreyfus and W. Kip Viscusi, "Rates of Time Preference and Consumer Valuations of Automobile Safety and Fuel Efficiency," *Journal of Law and Economics*, Vol. 38, No. 1 (April 1995), pp. 79-105. Even though we also estimate implicit rates of time preference as part of this article, we get value of life estimates more or less in line with those from the wage studies using a very large sample of used car purchases.

Page 7-25

I think I would distance myself very far from fudge factors like the 1.5 to 2 times higher value of morbidity than the out of pocket expenditures. I expect the range is actually huge.

Also, I noticed there was a missing component to this whole morbidity discussion in that no contingent valuation type morbidity studies ever came up. This is quite surprising, since EPA has funded a number of them. See Table 7 of my 1993 JEL piece which includes 8 articles on this topic.

Page 7-27

The use of conjoint analysis is not new to the field. See my article with Wesley Magat and Joel Huber, "Paired Comparison and Contingent Valuation Approaches to Morbidity Risk Valuation," *Journal of Environmental Economics and Management*, Vol. 15, No. 4 (December 1988), pp. 395-411. The paired comparison analysis used conjoint methods. As part of this analysis, we also test for the robustness of the results with respect to different conjoint metrics since the main issue from an economics standpoint is whether these metrics are in fact reflective of utility levels. Because there is no sound economic theory underlying the use of conjoint analysis, we used the conjoint approach to establish paired comparisons and points of indifference from which one can derive tradeoff values that have economic content.

Baruch Fischhoff prefers the spelling of his name with 2 H's rather than one. I was stunned to see contingent valuation handled with only a single paragraph. Perhaps 10 pages of discussion with the pros, the cons, the NOAA panel, etc. would be useful.

Page 7-28

It is there that you point out that the application of conjoint analysis is "very recent." Since our aforementioned analysis was in 1988 I wouldn't call that recent. That study by the way was funded by EPA.

Page 7-33

The Miller 1990s study is completely devoid of academic merit. I would rephrase this sentence as: "In addition, Miller's results are flawed because they are dependent on arbitrary and unreported weight adjustments he makes to wage risk data." I don't even think I would include this in the text. Perhaps a footnote. By the way Viscusi (1993) cited above is a bit more comprehensive than Viscusi (1992).

Page 7-34

I would take this Jones-Lee study with somewhat of a grain of salt. First, this is a contingent valuation study. Second, Jones-Lee's studies often have results that are not that robust with respect to the life cycle effects. I am publishing his latest in the next issue of JRU. Thus, I would at least treat this study with substantial caution and add appropriate caveats to it.

Page 7-35

What about these difficult equity issues? At any point in time the concern with income redistribution between the rich and the poor clearly raises difficult ethical issues, as I note in Viscusi (1992). However, it is this same economic influence that accounts for the rising valuation over time in the value of life and the environment, as future generations become richer. Indeed, it is because future generations richer that their valuations of these environmental benefits rise over time. Chapter 6 eagerly took advantage of this increased value of the environment with higher income levels over time, and it is inconsistent to not recognize similar differences at a point in time. Presumably income differences should count consistently throughout the report or they should not count in the report. As a practical matter, I believe that if EPA uses value of life numbers and makes appropriate quantity adjustments that that will be sufficient progress to have been made on this topic without getting into the thorny equity issues of whose life is worth how much. I usually say that the rich may value their lives more, but we will not draw any income distinctions, recognizing that this may be an implicit form of income redistribution.

A final note on quality-adjusted life years. This literature does not have the same kind of strong economic underpinnings as does the willingness-to-pay literature. Many times the quality adjustments are very ad hoc and certainly would not pass muster based on the usual standards applied to contingent valuation studies and the role of economic principles in guiding such estimates. Many of the quality adjustments in the literature are simply based on doctors' assessments of what percentage of a patient's welfare has been lost with certain ailments, which is certainly not an acceptable approach from a benefit standpoint. Other studies interview patients, but are not grounded in the economics of the individual choice. In cases in which the studies are done correctly, they will simply be variants of the willingness to pay studies that we are quite willing to accept for valuing morbidity, thus establishing what Dale Jorgensen observed would be a new baseline from which we can judge the benefits of mortality reduction.

Regards,
Kip

ATTACHMENT H-1
Written Comments of EEAC Members

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Dr. Bockstael

*

----- Forwarded Message Follows -----

From: "nancyb@arec.umd.edu" <nancyb@arec.umd.edu>

Subject: SAB meeting

Date: Tue, 17 Nov 1998 10:56:26 +0100

Rob -

Please don't feel you need to introduce all these points for discussion, by any means, but you may wish to turn these in to the EPA staff who are writing the guidelines for their consideration. While I haven't had time to read the entire document, this draft seems better than the first, but there are still some problems. Given what I perceive to be the importance of these Guidelines, it's probably worth the effort to get them into good shape. Here are some specific comments on Chapter 7. Some may seem nitpicking, but have more far reaching implications than may be obvious at this time.

Thanks. Nancy Bockstael

pg. 7-2. The statement is made that WTP and WTA will be close if there are small income effects. As Hanemann has shown, this is true for price changes, but the Willig results are not exactly applicable for quantity (or quality) changes. Even if income effects are small, if the good has no close substitutes, income flexibility can be large and differences can be large. Environmental quality changes generally take the form of quantity or quality and not price changes.

pg 7-2. The statements about consumer surplus are odd. No connection is made between CV/EV and CS. The authors seem to suggest that CS is an aggregate concept and one that applies only to market goods. Neither is necessarily true. CS is simply a "money held constant" rather than a "utility held constant" concept, and is the usual thing measured even in non-market contexts and for individuals.

pg.7-9. I have always had an unreasonable prejudice against the kind of pigeon-holing of "valuation approaches" listed in this table. It suggests that all revealed preference valuation approaches are idiosyncratic and stem from different underlying concepts of welfare. I won't argue strongly here, but I don't like it. It would be more effective to motivate the revealed preference methods from a household production perspective, then they don't look as though we're changing the concept of welfare to fit the problem.

pg 7-14. The authors argue that in the long run, producer surplus will disappear in competitive markets. This statement seems to suggest two things that are not really correct. The first is that most markets are perfectly competitive. The second is that there can be no rents or quasi-rents in competitive markets in the long run.

pg 7-17. The statement is made that discrete choice (or random utility models) are used when there are simultaneous environmental changes at a number of sites. While they can be used in this context, this is not the motivation for using them. The real reason to use random utility models is to get observations on behavior in the face of variation in environmental quality. Systems of demands don't typically work because site quality doesn't change over people at a site.

pg 7-18. While some of the confusion between marginal values and values for discrete changes has been cleared up in this draft, some still remains - particularly in the hedonic and averting behavior sections. For example, at the top of this page, to make it clear, this should read "reveal the marginal values they attach".

pg 7-21 through 7-22. The section on hedonics is still a bit shaky. The frequent reference to the second stage and the statement that "data limitations" prevent moving to the second stage are problematic. But, in fact, only if you have multiple market data or something of that sort is the "second stage estimation" meaningful in any way. The problem is not a data problem. The problem is that the hedonic reveals information only on the marginal valuation of anyone, and there is not way to identify anything else about individual's preferences for the environmental quality. (There is now much literature on this identification problem, but unfortunately the early literature was either wrong or misleading. It would be a shame to further propagate these errors.)

Pg 7-23 through 7-24. The same can be said about the averting behavior section, although here the problems are worse. The literature in this area has been very confused. The authors' discussion of the "full averting behavior model" suggests a misunderstanding. The sorts of terms present in the Harrington and Portney paper (pain and suffering, defensive expenditures, medical expenditures, lost wages, etc.) come from totally differentiating the LaGrangian (the individual's constrained utility maximization problem) with respect to the environmental quality. However, one can not just integrate each of these terms over the discrete change in quality to get a welfare measure. In addition, the authors suggest that this measure is the truth, while changes in averting behavior is something "less", but application of the envelope theorem illustrates that many of the terms in the H-P expression cancel out. My point here is that this section needs to be rewritten if it is to be this specific.

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Dr. Burtraw

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To: Robert Stavins
From: Dallas Burtraw
Comments on "Guidelines for Preparing Economic Analysis" (Nov. 3, 1998 draft)
November 23, 1998

Overall the "Guidelines" is a very nice document that promises to be a nice reference point for analysts. I focus my comments on areas for improvement, leaving aside the many opportunities for complements.

Chapter 5: Overview

This chapter is supposed to address cross-cutting issues. I feel the following issues should be addressed in this context.

1. Tax interaction effects

Evaluations of regulatory policy typically are set in a first-best regulatory setting. However, preexisting taxes such as taxes on labor income create a second-best setting. Recent advances in applied general equilibrium analysis have led to generally replicable results (at least in their qualitative nature) that indicate the direction of bias underestimating the cost of compliance, from a social perspective. However, this varies by regulation. However, a similar bias may exist with respect to benefits.

2. Baseline issues

The chapter, and indeed the entire document, does not provide guidance with respect to how forthcoming but as-of-yet not final regulations are to be treated in the baseline for evaluation of a specific policy. At the meeting of the EEAC I suggested that EPA practice was to only model regulations that were already final. One justification for doing so is to prevent abuse. However, I have been corrected, and informed that sometimes, forthcoming regulations are part of the baseline, which seems wise in most circumstances. In any case, there is no indication in the Guidelines as to standard practice.

3. Time

Time enters an analysis in several ways, but little attention is paid to any of them in the Guidelines. One way that time matters is in the pace of exogenous technological change. In principal this can either increase or decrease marginal and total abatement costs, depending on the direction of change, but in general we would expect it to decrease abatement costs. Recent analyses have indicated the magnitude, even for mature technologies, can be striking. See: Ellerman (recent Energy Policy) and Carlson (http://www.rff.org/disc_papers/PDF_files/9844.pdf) for studies pertaining to changes at coal-fired power plants.

A second way time matters is innovation induced by a regulation. Chapter 5 would seem like a good place to review the literature on regulatory incentives for innovation.

Chapter 6: Social Discounting

While I remain somewhat agnostic on this controversial area, the authors may want to see the recent article by Marty Weitzman in the most recent JEEM for evidence on behalf of the argument put forward in the chapter.

Chapter 7: Analyzing Benefits

1. Coordination

Although I refrain from drawing attention to good passages in general, I must draw attention to lines 18-24. This call for coordination is extremely important. Too often legislators have stated that recent analysis fails to address options that are being considered actively.

2. Mortality Valuation

At the EEAC meeting I argued that adjustments of the nature introduced by John Graham are desirable. However, I went on to state evidence supports Al McGartland's contention that

individuals do not value their remaining year(s) of life in a manner that is independent of their reference point. (This is true independent of changes in wealth that occur over a lifetime.) In other words, I disagreed with the suggestion that an "adding-up condition" would apply and each year of longevity should be treated equally. I invoked consumer sovereignty as justification for taking WTP of a 69 year old for one year life extension at "face value."

The overarching point is that valuation of lost life years should depend on the context of the injury. Imagine the case of a one-year extension in life, conditional on achieving 69 years of age, stemming from reduction in chronic exposure to particulates. In this case, neither the application of a VOSL derived from a prime age male's workplace choices, or the VOSL of a 69 year old for a one year life extension, reflect the proper context for opportunity costs that should be considered in the valuation. Assuming the chronic nature of the injury, to avoid the injury would incur costs over a 70 year lifetime. Each year, the value that an individual would be willing to pay could be presumed to vary. This argues against using a single number in the first place. But if a single number is all we have, then adjustments are desirable, but they should not be applied in a linear fashion.

Three studies come immediately to mind to make the point that the WTP for a life-year can be expected to vary with age.

- (1) Jones-Lee illustrates that there is an age-sensitivity that should be taken into account, but clearly it is not proportional to expected longevity.
- (2) Johannesson and Johannsson (1997) point out that that the expected quality of life in old age is much less than the value that is assigned by people in old age.
- (3) Murray and Lopez (1996) (from public health) offer another example in which the value of a year of life (holding health status otherwise constant) is not treated constant.

In summary, I agree with the implication of John Graham's presentation that adjustments are important and desirable, but I argue they should not be applied in a linear fashion.

Furthermore, as Graham and Richard Revesz argued, there are justifications for adjustments that head in the positive direction. Leading among these is the involuntariness of environmental risks.

Finally, let me highlight one realm for disagreement. Cropper et al (1994) that individuals would prefer to allocate resources to saving lives of the young at a ratio *greater* than the proportion of life-years remaining. This is interesting, but as long as benefit-cost theory adheres to consumer sovereignty in a rigorous manner, the WTP of individuals at a given reference point is the determinant factor. Hence, though environmental economics may have some catching up to do with public health, the distinction between allocating resources in a public health context and the practice of benefit-cost analysis in environmental management is a meaningful one and we should not expect it to disappear.

Chapter 8: Analyzing Social Costs

1. Partial equilibrium analysis

The presumption here is that partial equilibrium analysis is acceptable, and perhaps preferable, given resource constraints. I suggest the writing be a bit more equivocal about this.

The main problem I had with the text in this regard was the attempt to establish equivalency between compliance costs and social costs. The two may be proximate, but they are never synonyms. The text should be cleaned for where the terms are used interchangeably.

Second, the text suggests that the greater the opportunity for substitution the lower would be social costs compared to compliance costs. One reason this may not be true has to do with the type of policy being implemented. A technology standard does not allow for the same opportunity for input substitution as does an emission rate standard (Page 8-9, lines 1-10). Also, there is reason to think things might go in this direction, but they could well go in the opposite direction. This is likely to vary over time.

2. Time

Hazilla and Kopp (JPE) and papers by Jorgenson have established the time profile of social costs. In Hazilla and Kopp, social costs were less than compliance costs due to the opportunity to substitute among inputs in production. However, over a longer time horizon it was recognized that environmental compliance diverted investment capital from its highest valued use and set the economy on a different growth projectory that magnified social costs over time.

A second way that time matters is through innovation and dynamic efficiency. In this section there should be some discussion about innovation and how it might be expected to affect social costs (this should be foreshadowed in Chpt 5, as I suggest above).

3. Tax interaction effects

Tax interaction effects offer a reason why social costs may be systematically greater than compliance costs. In particular, the last sentence of the conclusion should be revised. See also pages 8-14 (CGE models are relevant even in the absence of a double dividend. See Parry, JEEM, 1995), 8-17 (where the discussion of marketable permits at lines 5-10 dismiss social costs – see Goulder, RAND Journal, 1996) and 8-21 (last sentence).

4. Provision of compensation

Compensation can affect avoidance behavior by creating adverse selection and/or moral hazard problems. This is a central idea of Coase. Text should be addressed at page 8-20.

5. Minor edits:

Page 8-3, line 19: Insert word “marginal”Section 8.4.1 The terminology in this section is unfortunate. 8.4.1.1 is labeled “Performance Standards” but actually seems to describe “technology standards.” Consider BAT and BPT. A performance standard is a term that fits emission rate standards, where there is some meaningful choice among investments that might be made to achieve that rate.

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Dr. Cameron

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TO: Robert Stavins
FROM: Trudy Ann Cameron
RE: detailed comments on "Chapter 7: Analyzing Benefits"
DATE: November 20, 1998

I will go line-by-line through my marginalia on the draft of the Benefits chapter, making suggestions as I go. The minutiae will be included as well. While I would like to devote more time to this, there is a binding constraint. I think this hits all the high points. Where some of my concerns overlap substantially with those of Nancy Bockstael, I forgo pointing them out again, since her comments will presumably be part of the record.

p. 1, line 45: "reveal" not "revel"

p. 2, line 21-23: WTP may or may not be easier to measure and quantify. I have most often heard it advocated as being "a conservative measure of value" rather than "the right measure of value." Since it is almost always less than WTA, it is more palatable to people who would prefer that the benefits be small.

p. 2, line 35-38: consumer surplus can be estimated from individual fitted demand curves as well as from aggregate market demand curves. This needs to be acknowledged at the beginning, although it is probably OK to emphasize value estimates from overall market demand curves, since these are often used. In line 37, it should probably say "how much of the good is demanded in the aggregate at each price".

p. 3, lines 10-13: this gives the impression that elasticities are sufficient to derive changes in surpluses. Only constant elasticity demand curves (e.g. log-log models) have just one elasticity that is relevant everywhere. It would be more appropriate to acknowledge that you need to know the configuration of the demand (or supply) function in the relevant region, not just some summary statistic such as demand (or supply) elasticity.

However, it is important to remind non-specialists that a common naïve assumption is to think that quantities traded will be unaffected by price changes (i.e. zero elasticity). This may not be valid.

IN GENERAL: There needs to be greater priority associated with benefit transfer, what it is, how it should be done. This is likely to be the main way any of this gets implemented, and it will be more valuable to inform the consumers of this document about how to choose between existing studies as candidates for benefits transfer than to teach them how to conduct their own original studies.

p. 7, line 32: explain what a "cost analyst" is. I'm not sure, for one.

Footnote 5: Car[c]inogen Risk Assessment

p. 8, line 40 "Guiding principle[s]..."

p. 8, lines 47-49: Awkward to read. Perhaps "This section describes the [categories] of benefits that are typically associated with environmental policies. [These descriptions are provided with understanding that quantification of benefits in each category will typically be desirable and should often be attempted.

However, the different possible techniques for accomplishing the quantification of the values of these benefits will be reserved for a later section.]”

Exhibit 7-1: It is risky to compartmentalize the different valuation methods as appropriate for different types of services flows. There is a lot of redundancy across benefit categories in the Human Health part. Non-market methods (e.g. stated preference) can supplement market methods for ecological benefits, to extend the domain of demand functions beyond the range observed in the historical data.

p. 11, line 15: “characterizations of a behavioral response to illness.” Perhaps there is some merit in commenting upon the use of subjective responses as opposed to objectively measured effects. Some readers might comprehend the idea that these characteristics of morbidity are themselves endogenous, they do not constitute exogenous shifters of willingness to pay for mitigation.

p. 11, line 46: I suspect it is Crock[er] and Shogren (1991).

p. 12, line 11: “..three steps ...[were] discussed...”

p. 12, line 19: “...focusing on those service flows [that/which?] are likely...” (I can never remember the right usage.)

p. 12, line 23: “Not only [is it] useful as a conceptual...”

p. 12, line 38-39: This is awkward. If the idea is “rivalry in consumption ,” perhaps that could be used explicitly. Anybody with Econ 1 should have heard about the problems with public goods. What is meant by “..benefits received by any one individual tend to affect benefits accruing to others.” In what way? Decreasing them? Enhancing them?

p. 13, line 7: Define the concept of an “instrumental” benefit. This is the first encounter with the term in the document. Perhaps the next sentence defines the term, but this role of that sentence is not made clear.

p. 13, line 21: can be expected to include [the] magnitude of [the] effect (and so on). Or, use magnitude-of-effect as one “thing.”

p. 13; line 38: elaborate a little on what is intended by “household soiling.” Dirty paint? Dirty roof shingles? Dust tracked into houses?

p. 14, line 34-37: the discussion of profits being competed away is just too pat. When have we ever seen a perfectly competitive industry? (If you stick with the current wording, line 36 should refer to a perfectly competitive [industry] (not a “model”).)

p. 14; line 44: “flushed out” seems a n incongruous term to use here.

p. 14, line 46: Mention what a “damage function approach” might be....

p. 14, line 48: Mention elasticities, cross-price elasticities, and substitution possibilities. This came up earlier, and might be referred-back-to.

p. 15, line 9: “Factors that influence the complexity...” In this context, the reader has a good chance of thinking that you are referring to factors of production, the previous use of the term “factors.”

p. 15, line 10: hyphenate “single-product” and “multi-product”

p. 15, lines 12-18: Make it clear whether you are talking about an increase or a decrease in environmental quality. This passage will be a little hard to follow for the uninitiated. Line 16: [other] factor prices?

p. 15, line 32: selection of [a] modeling approach

p. 15, line 35: Is it necessary to point out that “More-extensive treatment of consumer and producer responses is preferred to less.” Maybe that is not obvious, I suppose.

GENERAL: all kinds of valuation methods have systematic and familiar problems. For some reason, the longevity of a method seems lead people to judge it as more reliable, when this is not necessarily the case. Adequate caveats about the existence of a whole range of perils in all of the methods (not just CV) would be appropriate.

p. 16, line 22: Some recreation demand models are suited to new site assessment (notably a RUM model with McFadden’s conditional logit format and no simply respondent-specific explanatory variables). Others, like the travel cost model without any hedonic dimension, are less appropriate. They can handle decreases in access to existing sites, and might be stretched to predict increases in trips by current users, but cannot handle recruitment of entirely new users or be transferred to different sites with a different set of characteristics.

p. 16, line 26-27: There is some incongruity in this sentence The issue of “target species” crops up even though fishing or hunting have not been mentioned explicitly before. Not all recreation involves bagging wildlife. Also, is it intended to say “difficult [for the researcher] to observe” or “difficult [for the subject] to observe. It matters.

p. 16, line 35: and the [relationship among the variables] may be [interpreted] as a demand function

p. 17, line 11: There is an appropriate citation for this. I will try to find it. Kealy and ?

p. 18, line 26: why the “speed” of the work is relevant needs to be explained a little...

p. 18, line 31: workers’ perception[s]

p. 18, line 32 the resulting estimates of [the] compensation. There should be some comment about why workers generally underestimate on-the-job risks. This could be something about perception that would apply to any randomly selected individual. However, it is often the case that individuals self-select into risky jobs based on the disutility they experience from exposure to these risks. For example, many chemists swear “chemicals” are not bad for you, when ordinary people would be much more fearful of “chemicals.”

Footnote 8. Explain repeat sales analysis, however briefly.

p. 19, line 16: is this intended to say that there are estimated risk values for a variety of specific types of risk? Right now it is ambiguous, in that it may seem that the risk values are substantial.

p. 19, line 25-26: How is it that the injuries are often not defined specifically enough? Is a broken arm not a broken arm? An example of a typical ambiguity would help here, since the issue has been brought up.

p. 19, line 31: when mentioning the “number of difficulties” include parenthetical reference to one or two of them. Are these things like colinearity problems? To what does this mention refer?

p. 19, line 43: give an example of an occupational disease.

p. 20, line 1: can this simply be identified as a “self-selection bias” problem

p. 20, line 9-14: actuarial data to determine the risk levels faced by workers [in different occupations]. Elaborate on the idea of a fatality risk “off the job.” Would these be constant across workers, or correlated with occupation?

p. 20, line 45: does not match individuals’ perceptions, then the results of the analysis [may] be biased.

Be consistent: “a hedonic” or “an hedonic”

p. 21, line 11: Studies have used data on transaction involving both structures and land exclusively. This is ambiguous. How can you use both, exclusively? This seems to contradict.

p. 21, line 19: The information and modeling requirements of these states [differ] tremendously.. When you get to “data limitations often preclude the second stage of analysis,” I would have expected a little more discussion. For one thing, demand functions correspond to individuals, not to houses, so we need to gather additional data on buyers of houses, including their incomes and the choice set they faced when they made the decision. There are all kinds of thorny problems in the process of getting from an hedonic price function to anything that looks enough like a demand function to allow for welfare calculations.

Footnote 11: For example, “this holds” if few properties... What holds? Also, what is an equilibrium price function? Presumably, it is some reduced form of the market demand and supply functions. This makes it sound like something specific and different from the “hedonic price function.”

p. 22, line 4-8: This is awfully vague and sketchy. (first, line 5 is missing ...[the] analysis). The data limitations that are described seem only to be errors in measurement. These things seem advertised to affect only the second stage of the modeling process.

p. 22, line 20-22: One cannot overlook the possibility of lost “interim” value, even if snapshots of a housing market in equilibrium prior to some environmental insult or regulation, and sufficiently long after the incident or regime change, show relatively little loss of value. We don’t want to be misled by variations over time in value, but we do not want to discount these completely, either.

p. 22, line 30: Rather than using the potentially technical term “limited priors” (in the Bayesian statistical sense) I would suggest “little guidance.”

p. 22, line 39: Mention what “market segmentation” is.

p. 22, line 41-42: If you are not going to explain the intuitive rationale for the bias and efficiency results, provide a citation for these findings.

Footnote 12. Are fecal coliform counts observable to the consumer? I.e. can you see them in the water? Or just smell them at sufficient concentrations? Any more than any other water content data.

p. 23, line 1-3. Beware of advocating R-squared as the objective function in fitting an hedonic price function. “Stable” results in what sense? Derivatives that are robust across alternative specifications?

p. 23, line 9: “...included in this set.” Which set?

p. 23, line 22: What is meant by “a continuous relationship”? ...small reduction in health[,] or health risk,...

p. 23, line 35: ...lower bound [on] willingness to pay.

p. 23, line 39: If estimating [a] full averting behavior model...

Footnote 13: in what sense is the impact of changes in risk on self-protection expenditures “ambiguous”? (This may just be an awkward exposition.) Insignificantly different from zero, or significantly different from zero in opposite directions under different specifications with the same data.

p. 24, line 23: “reduced soiling of materials” perhaps some amplification at the time of the prior reference to this effect would save people from wondering what this is. What materials? How?

p. 24, line 29: I wonder if this means that we simply observe whether or not people are willing to incur costs at different levels to reduce risk. This makes it sound a lot like the same empirical environment as referendum CV analysis. Could you not infer WTP by observing the binary choices of a lot of people with similar preferences, for different levels of averting cost?

p. 24, line 41: ...Averting Behavior_ Studies

p. 25, line 10: Separability [from] other benefits? ...Are there likely to be instances when mitigation involves other related costs, rather than other related benefits?

p. 25, line 24: “simply summing the realized costs...” By this, do you mean “explicit market” costs, as opposed to nonmarket or implicit costs?

p. 25, line 30: It seems a bit oblique to say that the “theoretical basis for the cost-of-illness method is very limited.” The two assumptions mentioned may be “broadly consistent with neoclassical economics,” but are they anywhere near “broadly consistent with the real world”?

p. 25, line 38-39: altogether, not all together

p. 25, lines 49-50: ...this difference may vary greatly across health effects [and across individuals]. Understanding WTP requires knowing quite a lot about an individual’s preferences, which can vary dramatically from individual to individual.

p. 26, line 1-3: If you are going to introduce the distinctions among ways of valuing lost work hours, there needs to be a rationale.

p. 26, line 6: panel of physicians to develop a [generic] treatment profile

p. 26, line 8-9: data availability (data on what?)

p. 26, line 17: ...and data are often readily available... This appears to somewhat contradict the assertion above that data availability is often a determinant of what approach is used.

p. 26, line 24: by “expected averting costs,” do you imply statistical expectations, or just “typical” averting costs.

p. 26, line 26-27: seems redundant with footnote 14, page 24.

p. 26, line 31-32: Although the approach [may] account_ for costs... ... or [any costs that may have been incurred in order to avoid] the illness.

p. 27, line 1-3: It would help to emphasize that the method is NOT individual specific. The fact that the “wage rate chosen should reflect the demographic distribution of the illness under study” is less confusing when readers can remember that this is not individual data.

p. 27, line 6: Lost value of leisure time... (To whom, the individual who is sick, and/or their families? Other’s may value the individual’s leisure time as well.)

p. 27, line 17. Use “likely choices in a hypothetical market” rather than “preferences for a hypothetical good.”

p. 27, line 35: bias can be introduced easily into these studies if they are not carefully done (or even if they are!)

p. 27, line 41: Surveys without these tests [should be suspect;] surveys whose results fail these tests [may be] discredited.

p. 27, line 45: make choices between two [or more] (CA)....

p. 27, line 49: Multiple choice conjoint analysis is a take-it-or-leave it choice as well. There are simply more alternatives to the “status quo” choice. Since the information presented to the respondent is richer, their choices under these circumstances contain more information. ... “presented in the latter” is confusing. Which method is the “latter”?

p. 28, line 18-19: Since you bring this up.... It is not valid simply to assume that the nonuse values enjoyed by current users are equal to the total willingness to pay of nonusers. The reason, again, is sample selectivity. People who choose to be users may have fundamentally higher values for the resource than definite non-users. If I may be so bold, T.A. Cameron and J. Englin (RAND Journal of Economics, volume 28, no. 0, Special Issue ’97, p. S45-S70) makes this point explicitly. Paper title is “Welfare Effects of Changes in Environmental Quality Under Individual Uncertainty About Use”

Another relevant paper is "Respondent Experience and Contingent Valuation of Environmental Goods" (Trudy Ann Cameron and Jeffrey Englin) *Journal of Environmental Economics and Management* 33(3), 1997, 296-313. In this, we show that prior experience with a good (including "none" implying non-user status) has a statistically significant effect not only on expected WTP for an environmental good, but also on the noise in estimated values (furthermore, experience and values are modeled as jointly endogenous variables).

p. 28, line 25-26: I am always troubled by the assertion that "WTA applications of CV are much more problematic because, unlike the case of WTP, there is no budget constraint." This is technically incorrect. There is a budget constraint, there is simply no upper limit on the size of the opportunity set that defines compensating variation, whereas current income is an effective upper limit on the equivalent variation that would correspond to the same loss of environmental services. Differences between WTA and WTP are an artifact of people's preferences (the shapes of their indifference curves). It is perfectly possible that people will have indifference curves that are so far from "vertically parallel" that WTA will be vastly larger than WTP. Preferences are what they are, and consumer sovereignty demands that we respect these preferences. Where the problem lies is typically in the potential for strategic overstatement of WTA. The elicitation method can sometimes be faulted, but not people's preferences.

p. 28, line 31-34: "...and the problem that many respondents display intransitive preferences over the numerous, and often complex, set[s] of choices." Fortunately, myself, a student (and a colleague) are all working on these very issues. Namely choice set complexity and its effects on the preferences that are implied by CA studies. Much progress has been made over the last couple of years in the assessment of the effects of complexity on the error dispersion in random utility models of this type. Heteroscedasticity is a widespread problem, as a result of this complexity.

p. 28, line 36-39: it would be helpful to review the ordering of mail, telephone and in-person surveys by cost-per-observation and by their typical shortcomings.

IN GENERAL: It would be appropriate to note that contingent valuation has been very controversial in the past, in part because of its relevance to some extremely high-profile legal cases in the environmental valuation context. It is perceived as being more problematic than other methods of non-market valuation, but one must remember that the older methods are also subject to serious problems when badly applied. Alan Randall had a nice paper a few years back detailing the shortcomings of the travel cost method, for example. In the same context, however, readers must be reminded that despite the discomfort of many traditional economists with stated preference data, it is the only game in town in many contexts (especially for exclusively non-use values).

p. 29, line 14: Theory never tells us which variables will be "significant." It tells us which variables are likely to be important determinants; we merely hope that the coefficients on these variables will be statistically significant in our empirical models.

p. 30, line 22: would reduce the uncertainty of the [current] benefits estimate.

p. 30, line 27: well[-]accepted steps

p. 30, line 50: "the baseline criteria" What is a "criterion" in this context???

p. 31, line 10 (and elsewhere). I think "judgment" has only one "e"

p. 31, line 17 “...allows the analysis to [identify systematic] variation in existing...”

p. 31, line 43: fatal [risks] have been valued

p. 32, line 38-40: Is this a blessing upon the assumption that preferences do not change, or that we have identified all relevant heterogeneity in risk preferences across different groups? Noting that there seems to be little interest in developing new wage-risk studies may imply satisfaction with currently available numbers, for all purposes. This may not be your intent.

p. 33, line 8-9: [In practice,] there is a lack of a clear consensus...

GENERAL: section 7.6.1.3 may convey the misleading impression that these 26 particular point estimates are “gospel” on the issue of value of life. In fact, there is no presumption that these 26 values are independent random draws from some common underlying distribution of values of life, so that taking the average (in any sense) will provide an unbiased estimate of the true but unknown underlying mean value of life across all situations. This is simply a measure of central tendency among 26 separate estimates of the value of life derived from a wide variety of different studies. No mention is made of weighting each of these point estimates by the inverse standard errors associated with them (for example) by the original authors. Noticing that a Weibull distribution happens to provide an apparently good fit to the simple “marginal” distribution of these 26 values is rather shaky statistical justification for using the mean of \$48 million for the “true” value of a statistical life.

At a minimum, one might contemplate using these 26 numbers in a regression meta-analysis, of the type used by Smith and Huang for hedonic air quality studies. Interesting explanatory variables for the point estimates that come from each study would include the method and the vintage of the study, for example (data that are available in the tabular presentation of these 26 values). I am sure there are more dimensions of heterogeneity in conditions across these 26 studies that might make a difference to the size of the point estimate that results. There will especially be differences in the precision of the value estimates (trusting, of course, that information on precision was reported in the original studies). Conditional means, for studies under different conditions, might be more informative for benefits transfer exercises.

It is somewhat troubling that in all other benefits estimation exercises, consumer sovereignty is paramount, but in the case of value of life, we bow to political correctness in refusing to allow the value of life to differ systematically with sociodemographic attributes of the group in question. I agree entirely that it would be uncomfortable to admit that the social value of persons of different gender or ethnicity may differ. For some reason, only systematic differences by age appear to be admissible, despite anti-age-discrimination legislation, followed by health status and perhaps income.

p. 34, lines 38-40: Some mention should be made of the obvious arbitrariness of “using 75 percent of the VSL value for incidence among individuals over 65 years of age” Otherwise, this may appear prescriptive.

p. 38, line 41: is this double-counting, or simply over-estimation?

p. 39, line 16: “Potential shortcomings should be noted [even] for those studies that are considered to be of sufficient quality...”

p. 39, lines 27-31: It should be noted that one should be careful to assess whether the benchmark studies used to bound plausible estimates are themselves biased.

p. 40, lines 10-12: Ambiguity would be resolved by using: “they would value change in health status [that are certain.]” and “when using values for health effects [under certainty] to assess the changes in risks...”

p. 40, lines 37-42: “[A]lternative approaches..... attention of late[. However,] the results of these studies..... not well grounded in economic theory[, nor are they] typically applicable to policy analysis.

p. 40, line 45: “the broad range of service[s]”

p. 41, line 41, line 49 Explain the acronyms “O&M” and “POTW”

ATTACHMENT H-2
Written Comments of EEAC Members

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Dr. Kling

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From: Cathy Kling <clk@headyhall1.econ.iastate.edu>
To: TOM MILLER <MILLER.TOM@epamail.epa.gov>
Date: 11/29/98 8:39am
Subject: guidelines for economic analysis

Tom, My only additional comment on the guidelines for economic analysis is that once the document is complete, it might be useful to consider providing an index.

Thanks, Cathy

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Dr. Sigman

*

To: Tom Miller
From: Hilary Sigman
Re: Comments on draft "Guidelines for Preparing Economic Analysis"

I generally admire the document, which handles concisely and clearly many complex issues. Some specific suggestions are below.

Comments on chapter 6:

1. My sense is that there is more support in the economics literature for using conventional rates in intergenerational analyses than the chapter indicates. The chapter seems to draw too stark a distinction

between the two cases. The logic of potential Pareto improvements accepted earlier might apply here and is dismissed more readily than a reading of the literature would suggest.

2. The final choice of a .5-3% range for the intergenerational discount rate appears to come from the IPCC report. It might be appropriate to acknowledge the source, since it provides more background on the derivation of these numbers.

3. The chapter occasionally suggests that uncertainty can provide a rationale for not worrying about discounting. But, even the most uncertain benefits and costs should be discounted because they may otherwise give a more misleading impression. Places where the text gives a counter impression:

(1) page 6-1 lines 35-38;

(2) page 6-27, lines 33-47.

4. On page 6-15, the discussion might make clearer that the relevant tax rates for the wedge between the two rates of return is the effective marginal tax rate and that it is somewhat difficult to determine this rate in aggregate.

5. Page 6-4, lines 28-31 suggests an approach that would avoid discounting benefits. This approach would be inappropriate if the benefits vary over time. The approach is explained in a way that makes this problem more clear on page 6-28. I would keep the latter discussion, but skip the former.

6. Typo: p. 6-6, line 21, "to."

Comments on chapter 9:

1. I think there is too much emphasis on market power (section 9.2.6.1.1 and 9.2.6.1.2). The current discussion seems to require a very wide ranging and in depth analysis of complex conditions as a starting point for *every* analysis. Market structure issues deserve mention, but they seem much less useful than supply and demand elasticities and that prioritization does not come out clearly in this document. The chapter should suggest more of screening procedure in which a detailed analysis of market power is not recommended unless there is good reason to think that market power is important and its implications can be well understood.

2. On page 9-16 lines 12-15, the chapter asserts that firms with greater market power will be able to pass through more costs to consumers. In fact, that's not clearly true. For example, a monopoly will not entirely pass through an increase in marginal cost to consumers, whereas a perfectly competitive industry will with perfectly elastic supply.

3. The discussion of impacts of compliance costs on prices (section 9.2.7) should distinguish between costs that affect firms' marginal costs and those that do not. Only the former can be passed through to consumers (unless there are some more complicated effects through the number of firms in the industry).

4. This chapter (or the previous one) might provide some guidance on estimating supply and demand elasticities econometrically from price and quantity data. It worries me that the specific methods discussed for finding elasticities are non-econometric (such as section 9.2.7.3), when a good econometric method would usually be preferable. In addition, it might be useful to clarify that a two-equation method (e.g., two-stage least squares) is necessary to identify elasticities correctly.

5. In equity analyses, the chapter considers only the comparison of annual income to the poverty line as a way of identifying low-income populations. We might also wish to consider measures of lifetime income. One quick way to do this is to look at low consumption groups, since consumption will track households' expected lifetime incomes better than annual income. For example, one can divide the population by consumption deciles and see how the lowest deciles fare. It will not always be possible to do these analyses (for example, the Census does not contain consumption data), but if one were to look at the incidence of an energy tax, for example, this approach might be useful.

6. I disagree with using physical sensitivity as an equity dimension (page 9-47, lines 9-13). It is obviously important in risk analyses. But unless we have some separate reason for thinking we should ordinarily treat these people as deserving extra weight, there is no reason to do so now. (People with asthma sound special, but what about smokers?)

7. Why treat high property tax jurisdictions as weak financially (page 9-35)? Perhaps they just desire a high level of services.

8. The chapter asserts that strong growth increases ability to pass through costs (p. 9-15). It is not clear that this statement is true: it depends on the elasticities.

9. Typo: p. 9-8, line 14, "The is."

Comments on other chapters:

1. Section 4.4.1, p. 4-2 line 44: Deposit-refunds are more like taxes than "specialized forms of subsidies." Their net effect is equivalent to a tax on items that are not returned for the refund.

2. Section 5.3.2: I was confused by the inclusion of compliance in the section on "baseline specification." I think this discussion is excellent, but would have considered it to belong under the section on "predicting responses to a new environmental policy."

3. Section 8.2.4: Many of the "temporary" effects, such as plant closures and resource shifts to other markets, can also be permanent.

4. Section 8.3.5.1: The chapter might make the recommended use of input-output methods clearer. Conventional input-output analysis is helpful in analyzing the distribution of costs (e.g., the analyses in chapter 9), but it is not clear to me that it is especially useful in evaluating the level of costs, unless combined with some of the other forms of analysis discussed here.

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Dr. Viscusi

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From: Kip Viscusi

To: DCFCH01.DCFCHPO1(MILLER-TOM)

Date: 11/23/98 4:14pm
Subject: Economic SAB Comments on Guidelines

Dear Tom:

Below are my comments on Chapter 7 of the "Guidelines for Preparing Economic Analysis" document. The only comments that I wish to make for the record are those pertaining to Chapter 7. I will do so roughly in the order in which the comments appear.

First, let me make note of a point that came up at the meeting in relation to Chapter 7, but I believe belongs in Chapter 6. The role of the latency period is really a topic that pertains to discounting. Since the health impacts involving a latency period do not occur until after the period of exposure, the appropriate time frame for discounting should be the time in which the benefits accrue, which is the time which the diseases are prevented, not the time of exposure. This should be made explicit in the discounting chapter. Now let me turn to Chapter 7 where, unlike my comments at the meeting, I will present them in the order in which they occurred in the chapter not in the order of importance.

Page 7-6

In addition to quantifying the physical effects, in the case of health effects it is important to quantify the age distribution of the effects. In particular, if there are lifesaving activities, what is the age and the current life expectancy of those whose risks will be reduced? Recognizing these influences is important due to the great variations in life expectancy loss based on the cause of death. From an economic standpoint, the appropriate procedure is to examine the discounted expected life expectancy effects, where the role of discounting in this case makes the role of such quantity adjustments less influential than if no discounting were undertaken. Such estimates for a wide variety of causes of death appear in W. Kip Viscusi, Jahn Hakes, and Alan Carlin, "Measures of Mortality Risks," *Journal of Risk and Uncertainty*, Vol. 14, No. 3, (May/June 1997) pp. 213-233.

Page 7-9

Here, as well as below, when you discuss morbidity risks, the cost of illness approach will be mentioned. This is very much a second best type of approach that is only worthwhile when good willingness to pay data are not available. Cost of illness measures only provide a lower bound on the benefits, and often not a very good lower bound. It should be noted that until 1982 when I got the Occupational Safety and Health Administration to switch to the value of life approach that formerly federal agencies valued death by the "cost of death," which was similar in that only the present value of lost earnings and actual medical expenses counted. Doing so undervalued life by a factor of 10. The same could be true of some illnesses that go beyond coughs and colds.

Page 7-11

Near the end of the page "Crock and Shogren" presumably is not an editorial statement on Tom Crocker's work.

Page 7-12

I don't believe that indirect benefits should be counted at all. If you were going to count such indirect benefits, then one also has to account for the indirect costs, in particular the contribution of the opportunity costs "to the provision of another service flow." Unless, calculating indirect

benefits is narrowly defined in a way that avoids an asymmetry in how you count benefits and costs, I don't see how you can justify including it.

Page 7-13

I thought the wording of "an individual's commitment to environmental stewardship" was noteworthy since it is often claimed that contingent valuation surveys give upwardly biased answers because they are just eliciting such a warm glow benefit value. In any event, at this point contingent valuation was very much on my mind, but this chapter says next to nothing about what is clearly a central benefit valuation technique for environmental effects. Why?

Page 7-18

I thought that these caveats with respect to worker data were overdrawn since a number of studies have analyzed quite specifically the role of worker risk perceptions. See W. Kip Viscusi, "Employment Hazards: An Investigation of Market Performance," (Cambridge: Harvard University Press, 1979) as well as the Viscusi and O'Connor article that you cite. More importantly, this document is very strong on the caveats with respect to the wage studies, which are far and away the best studies, while at the same time even more caveats are pertinent to the other kinds of market evidence that have been used to assess the value of life. One would get the impression from reading this report that wage studies have the most drawbacks, not the fewest.

Page 7-19

In terms of applying the benefits of the wage studies it is important to note what they pertain to. This is perhaps my most important comment on the chapter. First, wage studies involve acute accidental deaths, not cancer. Although it is noted on page 7-19 that occupational diseases are not included, we do not know that this causes any bias in the risk tradeoffs for acute injuries. The real issue then is whether the premiums for accidental deaths are the same as for the kinds of deaths prevented by EPA policy, such as cancer. Consider, for example, the results in my survey, W. Kip Viscusi, "The Value of Risk to Life and Health," *Journal of Economic Literature*, Vol. XXXI, (December 1993), pp. 1912-1946. In Table 7 we review the valuations for fatal lymphoma based on Viscusi and Huber (1991) and find has a \$4 million dollar value that is roughly comparable to the value for acute accidents * certainly within the bounds of error. Although EPA can and should refine over time the estimates of the benefit values for different causes of death, it does not seem that there is great variation due to this influence.

There is, however, an important variation with respect to duration of life lost. My series of papers with Mike Moore indicate that this factor is quite influential. How one should handle this as a practical matter is clearly an important benefit concern because the people at risk in the wage-job risk studies have an average age in their 30s with much more life to lose than many of the target groups exposed to environmental risks. Until more precise estimates are available of the differential value of life at different ages, I would propose that all ages be treated symmetrically in terms of the weight placed per discounted year of life expectancy. EPA reports should consequently show two sets of numbers - one based on a cost per life saved without any adjustments for age and a second estimate based on a normalized life saved using discounted life years for the normalization. Equivalently, it could be in terms of discounted life years saved.

Page 7-21

The number of analytical bridges that have to be used in property value studies to estimate values of life are enormous. Also, if it is assumed that workers have trouble figuring out job risks, how well do people do in terms of identifying sources of pollution and the effects of pollution on their lives? I would think that this is much tougher. Moreover, the only study in which direct estimates of the risk appear in a property value equation * as opposed to some other measure such as pollution levels * to the best of my knowledge is the unpublished work that I have done with Ted Gayer and Jay Hamilton, which we are not yet circulating. As an aside, I would have put the automobile price literature ahead of the property value literature in terms of there being better estimates. The previous studies in the literature are reviewed in my JEL piece as well as in Fatal Tradeoffs. The more recent, and the most comprehensive valuation estimate appears in Mark Dreyfus and W. Kip Viscusi, "Rates of Time Preference and Consumer Valuations of Automobile Safety and Fuel Efficiency," *Journal of Law and Economics*, Vol. 38, No. 1 (April 1995), pp. 79-105. Even though we also estimate implicit rates of time preference as part of this article, we get value of life estimates more or less in line with those from the wage studies using a very large sample of used car purchases.

Page 7-25

I think I would distance myself very far from fudge factors like the 1.5 to 2 times higher value of morbidity than the out of pocket expenditures. I expect the range is actually huge.

Also, I noticed there was a missing component to this whole morbidity discussion in that no contingent valuation type morbidity studies ever came up. This is quite surprising, since EPA has funded a number of them. See Table 7 of my 1993 JEL piece which includes 8 articles on this topic.

Page 7-27

The use of conjoint analysis is not new to the field. See my article with Wesley Magat and Joel Huber, "Paired Comparison and Contingent Valuation Approaches to Morbidity Risk Valuation," *Journal of Environmental Economics and Management*, Vol. 15, No. 4 (December 1988), pp. 395-411. The paired comparison analysis used conjoint methods. As part of this analysis, we also test for the robustness of the results with respect to different conjoint metrics since the main issue from an economics standpoint is whether these metrics are in fact reflective of utility levels. Because there is no sound economic theory underlying the use of conjoint analysis, we used the conjoint approach to establish paired comparisons and points of indifference from which one can derive tradeoff values that have economic content.

Baruch Fischhoff prefers the spelling of his name with 2 H's rather than one. I was stunned to see contingent valuation handled with only a single paragraph. Perhaps 10 pages of discussion with the pros, the cons, the NOAA panel, etc. would be useful.

Page 7-28

It is there that you point out that the application of conjoint analysis is "very recent." Since our aforementioned analysis was in 1988 I wouldn't call that recent. That study by the way was funded by EPA.

Page 7-33

The Miller 1990s study is completely devoid of academic merit. I would rephrase this sentence as: "In addition, Miller's results are flawed because they are dependent on arbitrary and unreported weight adjustments he makes to wage risk data." I don't even think I would include this in the text. Perhaps a footnote. By the way Viscusi (1993) cited above is a bit more comprehensive than Viscusi (1992).

Page 7-34

I would take this Jones-Lee study with somewhat of a grain of salt. First, this is a contingent valuation study. Second, Jones-Lee's studies often have results that are not that robust with respect to the life cycle effects. I am publishing his latest in the next issue of JRU. Thus, I would at least treat this study with substantial caution and add appropriate caveats to it.

Page 7-35

What about these difficult equity issues? At any point in time the concern with income redistribution between the rich and the poor clearly raises difficult ethical issues, as I note in Viscusi (1992). However, it is this same economic influence that accounts for the rising valuation over time in the value of life and the environment, as future generations become richer. Indeed, it is because future generations richer that their valuations of these environmental benefits rise over time. Chapter 6 eagerly took advantage of this increased value of the environment with higher income levels over time, and it is inconsistent to not recognize similar differences at a point in time. Presumably income differences should count consistently throughout the report or they should not count in the report. As a practical matter, I believe that if EPA uses value of life numbers and makes appropriate quantity adjustments that that will be sufficient progress to have been made on this topic without getting into the thorny equity issues of whose life is worth how much. I usually say that the rich may value their lives more, but we will not draw any income distinctions, recognizing that this may be an implicit form of income redistribution.

A final note on quality-adjusted life years. This literature does not have the same kind of strong economic underpinnings as does the willingness-to-pay literature. Many times the quality adjustments are very ad hoc and certainly would not pass muster based on the usual standards applied to contingent valuation studies and the role of economic principles in guiding such estimates. Many of the quality adjustments in the literature are simply based on doctors' assessments of what percentage of a patient's welfare has been lost with certain ailments, which is certainly not an acceptable approach from a benefit standpoint. Other studies interview patients, but are not grounded in the economics of the individual choice. In cases in which the studies are done correctly, they will simply be variants of the willingness to pay studies that we are quite willing to accept for valuing morbidity, thus establishing what Dale Jorgensen observed would be a new baseline from which we can judge the benefits of mortality reduction.

Regards,
Kip

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